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COMPANION TO THE OBSERVATORY, A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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TO

THE OBSERVATORY,

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1904.

VOL. XXVII.

INTRODUCTION.

THE present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Lœwy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations of stars by the Moon, the phenomena of Jupiter's Satellites, and the diagrams and Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.' The Ephemeris of Jupiter V. is from the 'Connaissance de Temps,' 1904.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used, and the astronomical day is reckoned from noon to noon, except in the Ephemerides of Variable Stars, where Greenwich Civil Time is used, the day being reckoned from midnight to midnight.

Date.	Day of Year.	Fr. of Year.	SUN		O's Dec. Noon.	M.T. at Apparent Noon.		Sid. T. at Mean Noon.	MOON'S PHASES.
			Sets.	Rises.		h m s	h m s		
1904.			h m	h m	o	h m s	h m s		d h m
Jan. 3	3	'006	4 1	20 8	22 56 S	0 4 8	18 46 46		Jan.
10	10	'025	4 9	20 5	22 7	7 13	19 14 22	○	2 17 47
17	17	'044	4 19	20 0	20 58	9 54	19 41 58	☾	9 9 10
24	24	'063	4 31	19 51	19 28	12 0	20 9 34	●	17 3 47
31	31	'082	4 43	19 42	17 42	13 28	20 37 10	☾	25 8 41
Feb. 7	38	'101	4 55	19 31	15 39	14 15	21 4 46	○	1 4 33
14	45	'121	5 9	19 19	13 24	14 24	21 32 22	☾	7 21 56
21	52	'140	5 21	19 5	10 58	13 57	21 59 58	●	15 23 5
28	59	'159	5 34	18 50	8 23	12 56	22 27 33	☾	23 23 9
Mar. 6	66	'178	5 46	18 35	5 43	11 29	22 55 9	○	1 14 48
13	73	'197	5 59	18 18	2 59	9 41	23 22 45	☾	8 13 1
20	80	'216	6 11	18 2	0 13 S	7 41	23 50 21	●	16 17 39
27	87	'236	6 23	17 47	2 33 N	5 33	0 17 57	☾	24 9 37
April 3	94	'253	6 34	17 31	5 15	3 25	0 45 33	○	31 0 44
10	101	'274	6 46	17 16	7 54	1 25	1 13 9	☾	7 5 53
17	108	'293	6 57	17 1	10 25	23 59 38	1 40 45	●	15 9 53
24	115	'312	7 8	16 47	12 49	58 9	2 8 20	☾	22 16 55
May 1	122	'331	7 20	16 32	15 2	57 3	2 35 56	○	29 10 36
8	129	'350	7 30	16 20	17 3	56 23	3 3 32	☾	6 23 50
15	136	'369	7 42	16 9	18 50	56 12	3 31 8	●	14 22 58
22	143	'389	7 52	16 0	20 21	56 28	3 58 44	☾	21 22 19
29	150	'408	8 1	15 52	21 36	57 9	4 26 20	○	28 20 55
June 5	157	'427	8 9	15 47	22 32	58 12	4 53 56	☾	5 17 53
12	164	'446	8 14	15 45	23 9	59 32	5 21 32	●	13 9 11
19	171	'465	8 18	15 44	23 26	0 1 1	5 49 7	☾	20 3 11
26	178	'485	8 19	15 46	23 23	2 31	6 16 43	○	27 8 23
July 3	185	'504	8 18	15 51	22 59	3 54	6 44 19	☾	5 10 54
10	192	'523	8 14	15 57	22 16	5 4	7 11 55	●	12 17 27
17	199	'542	8 8	16 6	21 14	5 53	7 39 31	☾	19 8 49
24	206	'561	7 58	16 15	19 55	6 17	8 7 7	○	26 21 42
31	213	'580	7 49	16 24	18 19	6 11	8 34 43	☾	
Aug. 7	220	'600	7 37	16 35	16 29	5 37	9 2 19	☾	4 2 3
14	227	'619	7 24	16 46	14 26	4 34	9 29 55	●	11 0 58
21	234	'638	7 10	16 58	12 11	3 4	9 57 30	☾	17 16 27
28	241	'657	6 55	17 9	9 47	1 12	10 25 6	○	25 13 2
Sept. 4	248	'676	6 39	17 21	7 15	23 59 1	10 52 42	☾	2 14 59
11	255	'695	6 24	17 31	4 38	56 40	11 20 18	●	9 8 43
18	262	'715	6 8	17 42	1 56 N	54 12	11 47 54	☾	16 3 13
25	269	'734	5 52	17 53	0 47 S	51 45	12 15 30	○	24 5 50
Oct. 2	276	'753	5 35	18 5	3 31	49 25	12 43 6	☾	2 1 52
9	283	'772	5 19	18 17	6 12	47 22	13 10 42	●	8 17 25
16	290	'791	5 4	18 28	8 50	45 40	13 38 17	☾	15 17 54
23	297	'810	4 49	18 41	11 21	44 26	14 5 53	○	23 22 56
30	304	'830	4 36	18 53	13 44	43 45	14 33 29	☾	31 11 13
Nov. 6	311	'849	4 24	19 5	15 57	43 43	15 1 5	●	7 3 37
13	318	'868	4 13	19 18	17 56	44 21	15 28 41	☾	14 12 36
20	325	'887	4 4	19 29	19 40	45 41	15 56 17	○	22 15 12
27	332	'906	3 56	19 41	21 7	47 39	16 23 53	☾	29 19 38
Dec. 4	339	'925	3 52	19 50	22 14	50 14	16 51 29	●	6 15 46
11	346	'945	3 49	19 58	23 0	53 17	17 19 4	☾	14 10 7
18	353	'964	3 50	20 5	23 24	56 38	17 46 40	○	22 6 1
25	360	'983	3 53	20 7	23 24 S	0 0 7	18 14 16	☾	29 3 46

For Moon's phases: ● signifies New; ☾ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.										FEBRUARY.										MARCH.										APRIL.									
Day.		Moon.				Day.		Moon.				Day.		Moon.				Day.		Moon.				Day.		Moon.													
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.										
1	F	18 43	10 46	18 19 N	-77°6 M.	1	M	5 11	28 12	42 N	+85°5 E.	1	Tu	5 10	12 3	6 33 N	-87°4 M.	1	F	8 01	26 9	18 S	+75°1 E.	1	F	8 01	26 9	18 S	+75°1 E.										
2	S	19 42	11 47	18 15	-89°7 M.	2	Tu	6 22	13 26	8 47	+73°4 E.	2	W	6 32	13 0	1 57 N	+80°5 E.	2	S	9 16	14 19	12 49	+62°9 E.	2	S	9 16	14 19	12 49	+62°9 E.										
3	S	rises	12 48	16 55	+78°1 E.	3	Th	7 44	14 22	4 22 N	+61°3 E.	3	Th	7 54	13 54	2 42 S	+68°4 E.	3	S	10 26	15 13	15 31	+50°7 E.	3	S	10 26	15 13	15 31	+50°7 E.										
4	M	6 14	13 49	14 24	+66°0 E.	4	F	9 15	16 9	0 14 S	+49°1 E.	4	F	9 13	14 48	7 4	+56°2 E.	4	Tu	12 27	16 57	18 7	+38°6 E.	4	Tu	12 27	16 57	18 7	+38°6 E.										
5	W	7 32	14 46	10 57	+53°9 E.	5	F	10 21	16 9	4 41	+37°0 E.	5	S	10 27	15 41	10 54	+44°1 E.	5	W	13 17	17 47	18 7	+14°2 E.	5	W	13 17	17 47	18 7	+14°2 E.										
6	S	8 51	15 42	6 51	+41°8 E.	6	S	11 36	17 0	8 45	+24°9 E.	6	S	11 39	16 33	14 1	+31°9 E.	6	W	13 17	17 47	18 7	+14°2 E.	6	W	13 17	17 47	18 7	+14°2 E.										
7	Th	10 10	16 34	2 27 N	+29°6 E.	7	S	12 47	17 51	12 13	+12°7 E.	7	M	12 44	17 24	16 18	+19°8 E.	7	Th	13 59	18 35	17 5	+2°0 E.	7	Th	13 59	18 35	17 5	+2°0 E.										
8	F	11 26	17 25	1 59 S	+17°5 E.	8	M	13 53	18 41	14 58	+0°5 E.	8	Tu	13 43	18 15	17 41	+7°6 E.	8	F	14 35	19 21	15 24	+10°2 E.	8	F	14 35	19 21	15 24	+10°2 E.										
9	S	12 39	18 15	6 12	+5°3 E.	9	Tu	14 55	19 30	16 55	-11°6 E.	9	W	14 36	19 41	18 10	+4°6 E.	9	S	15 72	20 18	1 1	-23°8 E.	9	S	15 72	20 18	1 1	-23°8 E.										
10	S	13 49	19 5	10 0	-6°8 E.	10	W	15 50	20 18	1	-36°2 E.	10	Th	15 21	19 53	17 48	-16°8 E.	10	Th	16 35	21 25	14 4	-46°6 E.	10	Th	16 35	21 25	14 4	-46°6 E.										
11	M	14 58	19 54	13 13	-19°0 E.	11	Th	16 40	21 8	18 15	-23°8 E.	11	F	16 02	20 4	16 38	-29°0 E.	11	M	17 30	22 54	9 17	-34°6 E.	11	M	17 30	22 54	9 17	-34°6 E.										
12	Tu	16 1	20 43	15 43	-31°2 E.	12	F	17 24	21 56	17 39	-48°2 E.	12	S	16 35	21 25	14 45	-41°2 E.	12	Tu	18 02	22 16	3 21 S	-59°1 E.	12	Tu	18 02	22 16	3 21 S	-59°1 E.										
13	W	17 22	21 33	17 26	-43°5 E.	13	S	17 59	22 43	16 17	-60°4 E.	13	S	17 42	20 12	16 16	-53°4 E.	13	W	19 10	23 45	4 12	-83°5 E.	13	W	19 10	23 45	4 12	-83°5 E.										
14	Th	17 55	22 22	18 18	-55°5 E.	14	M	sets		11 34	-84°8 E.	14	M	17 30	22 54	9 17	-77°8 E.	14	Th	17 10	23 45	4 12	-83°5 E.	14	Th	17 10	23 45	4 12	-83°5 E.										
15	F	18 43	23 11	18 18	-67°7 E.	15	Tu	6 35	0 12	8 27	+83°0 M.	15	Tu	6 32	0 20	1 29 N	+90°0 M.	15	F	8 41	1 48	8 47	+53°4 M.	15	F	8 41	1 48	8 47	+53°4 M.										
16	S	19 24	23 59	17 28	-79°9 E.	16	W	7 35	0 55	4 59	+70°8 M.	16	W	7 37	1 38	1 19 S	+58°6 M.	16	W	8 42	2 21	2 26 N	+46°4 M.	16	W	8 42	2 21	2 26 N	+46°4 M.										
17	S	sets		15 52	+87°9 M.	17	Th	8 40	2 21	2 26 N	+34°3 M.	17	Th	9 45	3 5	6 7	+22°1 M.	17	Th	9 45	3 5	6 7	+22°1 M.	17	Th	9 45	3 5	6 7	+22°1 M.										
18	M	5 39	0 45	13 36	+75°7 M.	18	Th	10 49	3 49	9 36	+22°1 M.	18	F	11 53	4 36	12 44	+9°9 M.	18	F	12 56	5 17	5 5	+4°5 M.	18	F	12 56	5 17	5 5	+4°5 M.										
19	W	6 38	1 30	10 46	+63°5 M.	19	F	12 56	5 17	5 5	+4°5 M.	19	S	13 52	6 01	6 18	-7°6 M.	19	S	14 40	7 37	9 46	-25°7 M.	19	S	14 40	7 37	9 46	-25°7 M.										
20	S	7 41	2 14	7 32	+51°4 M.	20	S	14 53	4 39	9 36	+22°1 M.	20	S	15 56	4 12	16 43	+16°7 M.	20	S	16 44	5 17	5 5	+4°5 M.	20	S	16 44	5 17	5 5	+4°5 M.										
21	Th	8 42	2 57	3 59	+39°2 M.	21	M	15 56	4 12	16 43	+16°7 M.	21	M	16 44	5 17	5 5	+4°5 M.	21	M	17 41	5 17	5 5	+4°5 M.	21	M	17 41	5 17	5 5	+4°5 M.										
22	F	9 45	3 39	0 16 S	+27°0 M.	22	F	16 16	5 7	11 18	-14°3 M.	22	F	17 11	6 17	7 58	-26°6 M.	22	F	18 01	6 17	7 58	-26°6 M.	22	F	18 01	6 17	7 58	-26°6 M.										
23	S	10 50	4 22	3 30 N	+14°8 M.	23	W	17 11	6 17	7 58	-26°6 M.	23	W	18 01	6 17	7 58	-26°6 M.	23	W	19 01	6 17	7 58	-26°6 M.	23	W	19 01	6 17	7 58	-26°6 M.										
24	S	11 54	5 6	7 12	+2°7 M.	24	Th	18 01	6 17	7 58	-26°6 M.	24	Th	19 01	6 17	7 58	-26°6 M.	24	Th	20 01	6 17	7 58	-26°6 M.	24	Th	20 01	6 17	7 58	-26°6 M.										
25	M	13 1	5 52	10 40	-9°5 M.	25	F	19 01	6 17	7 58	-26°6 M.	25	F	20 01	6 17	7 58	-26°6 M.	25	F	21 01	6 17	7 58	-26°6 M.	25	F	21 01	6 17	7 58	-26°6 M.										
26	W	14 8	6 41	13 44	-21°7 M.	26	S	20 01	6 17	7 58	-26°6 M.	26	S	21 01	6 17	7 58	-26°6 M.	26	S	22 01	6 17	7 58	-26°6 M.	26	S	22 01	6 17	7 58	-26°6 M.										
27	Th	15 15	7 32	16 11	-33°8 M.	27	Th	21 01	6 17	7 58	-26°6 M.	27	Th	22 01	6 17	7 58	-26°6 M.	27	Th	23 01	6 17	7 58	-26°6 M.	27	Th	23 01	6 17	7 58	-26°6 M.										
28	F	16 20	8 28	17 47	-46°0 M.	28	F	22 01	6 17	7 58	-26°6 M.	28	F	23 01	6 17	7 58	-26°6 M.	28	F	24 01	6 17	7 58	-26°6 M.	28	F	24 01	6 17	7 58	-26°6 M.										
29	S	17 21	9 26	18 20	-58°1 M.	29	M	23 01	6 17	7 58	-26°6 M.	29	M	24 01	6 17	7 58	-26°6 M.	29	M	25 01	6 17	7 58	-26°6 M.	29	M	25 01	6 17	7 58	-26°6 M.										
30	F	18 16	10 26	17 39	-70°2 M.	30	F	25 01	6 17	7 58	-26°6 M.	30	F	26 01	6 17	7 58	-26°6 M.	30	F	27 01	6 17	7 58	-26°6 M.	30	F	27 01	6 17	7 58	-26°6 M.										

MAY.				JUNE.				JULY.				AUGUST.			
Day.		Moon.		Day.		Moon.		Day.		Moon.		Day.		Moon.	
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.	Month.	Week.	Rises.	Souths.
h m	h m	h m	h m	°	°	h m	h m	h m	h m	°	°	h m	h m	h m	h m
1	S	9 13	52	16 47 S	+69° 1 E.	1	W	10 32	15 7	16 59 S	+50° 5 E.	1	M	9 49	16 7
2	S	10 15	45	18 1	+59° 9 E.	2	Th	11 8	15 54	15 3	+38° 4 E.	2	Tu	10 12	16 50
3	Tu	11 9	37	18 18	+44° 7 E.	3	F	11 39	16 40	12 30	+26° 1 E.	3	W	10 37	17 35
4	W	11 54	16	27 17 39	+32° 5 E.	4	S	12 5	17 24	9 28	+13° 9 E.	4	Th	11 6	18 22
5	Th	12 34	17	15 16 11	+20° 3 E.	5	S	12 30	18 7	6 3	+1° 7 E.	5	F	11 39	19 12
6	Fr	13 8	18	1 14 2	+8° 1 E.	6	S	12 53	18 50	2 24 S	-10° 5 E.	6	S	12 19	20 6
7	S	13 37	18	46 11 19	-4° 1 E.	7	Tu	13 16	19 33	1 24 N	-22° 7 E.	7	S	13 8	21 18
8	S	14 2	19	29 8 9	-16° 3 E.	8	W	13 40	20 17	5 11	-35° 0 E.	8	M	14 7	22 0
9	M	14 26	20	12 4 38	-28° 8 E.	9	Th	14 5	21 3	8 51	-47° 2 E.	9	Tu	15 14	23 59
10	Tu	14 48	20	55 0 55 S	-40° 7 E.	10	F	14 36	21 52	12 12	-59° 5 E.	10	W	16 40	23 59
11	W	15 12	21	39 2 54 N	-53° 0 E.	11	S	15 9	22 44	15 2	-71° 7 E.	11	Th	17 49	0 57
12	Th	15 38	22	25 6 40	-65° 2 E.	12	S	15 52	23 39	17 9	-84° 0 E.	12	F	18 23	1 53
13	F	16 6	23	12 10 13	-77° 4 E.	13	S	16 32	0 36	18 22	-83° 8 E.	13	S	19 20	3 42
14	S	16 38		13 23	-89° 7 E.	14	Tu	17 9	24 1	34 17 16	+59° 3 M.	14	S	20 3	4 26
15	S	sets	0 25 55		+78° 1 M.	15	W	18 24	1 34	17 16	+59° 3 M.	15	Tu	20 34	5 34
16	M	8 43	0 55	17 38	+65° 8 M.	16	Th	19 9	2 32	15 4	+47° 1 M.	16	W	20 24	5 27
17	Tu	9 44	1 50	18 23	+53° 6 M.	17	Fr	19 46	3 29	11 55	+34° 8 M.	17	Th	21 11	6 19
18	W	10 40	2 46	18 2	+41° 4 M.	18	S	20 11	4 24	8 4	+22° 6 M.	18	F	22 30	8 1
19	Th	11 27	3 43	16 35	+29° 1 M.	19	S	21 48	5 17	3 48 N	+10° 4 M.	19	S	23 22	8 51
20	Fr	12 7	4 39	14 3	+16° 9 M.	20	M	22 16	6 9	0 39 S	-1° 9 M.	20	F	24 10	9 40
21	S	12 42	5 34	10 50	+4° 7 M.	21	Tu	23 12	7 0	5 0	-14° 1 M.	21	Tu	25 18	10 27
22	S	13 14	6 27	6 54	-7° 5 M.	22	W	23 13	7 50	9 3	-26° 3 M.	22	W	26 16	11 13
23	M	13 43	7 19	2 35 N	-19° 7 M.	23	Th	24 13	8 42	12 22	-38° 5 M.	23	Th	27 16	12 14
24	Tu	14 11	8 11	1 52 S	-31° 9 M.	24	F	24 20	9 33	15 23	-50° 7 M.	24	Fr	28 16	13 12
25	W	14 39	9 3	6 12	-44° 1 M.	25	S	25 15	0 25	17 19	-62° 9 M.	25	S	29 17	14 5
26	Th	15 9	9 55	10 10	-63° 3 M.	26	S	26 15	46 11	18 18 20	-75° 1 M.	26	S	30 17	15 57
27	Fr	15 43	10 47	13 32	-68° 5 M.	27	S	27 15	48 12	19 25	-87° 3 M.	27	F	31 17	16 48
28	S	16 21	11 40	16 6	-80° 7 M.	28	Tu	28 8	28 12	59 17 34	-80° 5 E.	28	Tu	32 13	17 48
29	M	17 12	12 33	17 45	+8° 1 E.	29	W	29 9	7 13	48 15 55	+68° 3 E.	29	W	33 13	18 30
30	Th	18 58	13 26	18 26	+74° 9 E.	30	Th	30 9	41 14	35 13 35 S	+56° 1 E.	30	Th	34 15	19 26

SEPTEMBER.										OCTOBER.										NOVEMBER.										DECEMBER.									
Day.		Moon.				Day.		Moon.				Day.		Moon.				Day.		Moon.				Day.		Moon.													
Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.	Month.	Week.	Rises.	Souths.	Dec.	Long. of Termi-nator.										
1	Tu	9 39 17	5 14 28 N	+ 65 E.	+ 65 E.	1	S	9 43 17	36 18 19 N	0 5 E.	+ 05 E.	1	Tu	11 54 19	8 11 59 N	0 159 N	-172 E.	1	Tu	11 54 19	8 11 59 N	0 159 N	-172 E.	1	Tu	13 28 19	42 0 46 N	0 46 N	-223 E.										
2	F	10 14 17	56 16 35	- 57 E.	- 57 E.	2	S	10 40 18	36 17 57	-117 E.	-117 E.	2	W	13 9 20	2 8 10	0 8 10	-294 E.	2	F	14 45 20	34 3 46 S	3 46 S	-345 E.	2	F	14 45 20	34 3 46 S	3 46 S	-345 E.										
3	S	10 58 18	49 17 55	-180 E.	-180 E.	3	M	11 44 19	26 16 32	-239 E.	-239 E.	3	Tu	14 27 20	56 3 49 N	3 49 N	-416 E.	3	S	16 3 21	27 8 6	8 6	-467 E.	3	S	16 3 21	27 8 6	8 6	-467 E.										
4	M	11 51 19	44 18 16	-302 E.	-302 E.	4	Tu	12 55 20	22 14 4	-361 E.	-361 E.	4	F	15 46 21	50 0 49 S	0 49 S	-538 E.	4	M	17 19 22	21 11 58	11 58	-588 E.	4	M	17 19 22	21 11 58	11 58	-588 E.										
5	M	12 53 20	42 17 33	-424 E.	-424 E.	5	W	14 12 21	18 10 40	-484 E.	-484 E.	5	S	17 7 22	44 5 26	5 26	-660 E.	5	Tu	18 32 23	17 15 7	15 7	-710 E.	5	Tu	18 32 23	17 15 7	15 7	-710 E.										
6	Tu	14 32 10	40 15 42	-546 E.	-546 E.	6	Tu	15 32 22	14 6 31	-606 E.	-606 E.	6	S	18 25 23	40 9 42	9 42	-782 E.	6	W	sets		17 20	832 E.	6	W	sets		17 20	832 E.										
7	W	15 20 22	38 12 45	-669 E.	-669 E.	7	F	16 54 23	10 1 55 N	-728 E.	-728 E.	7	M	sets		13 21	+896 M.	7	W	4 42 0	12 18 31	12 31	+846 M.	7	W	4 42 0	12 18 31	12 31	+846 M.										
8	Th	16 41 23	36 8 55	-791 E.	-791 E.	8	S	sets		2 50 S	-850 E.	-850 E.	8	Tu	5 23 0	36 16 7	7	+774 M.	8	Tu	5 35 1	7 18 37	18 37	+724 M.	8	Tu	5 35 1	7 18 37	18 37	+724 M.									
9	F	17 17 11	32 3 20 S	-857 E.	-857 E.	9	M	5 45 0	6 7 22	+828 M.	+828 M.	9	W	6 56 2	32 17 52	17 52	+652 M.	9	F	6 32 2	17 43	17 43	+602 M.	9	F	6 32 2	17 43	17 43	+602 M.										
10	S	6 49 0	32 0 16 S	+764 M.	+764 M.	10	M	6 18 1	11 11 23	+583 M.	+583 M.	10	Tu	7 50 3	21 18 11	18 11	+408 M.	10	S	7 33 2	52 15 57	52 57	+480 M.	10	S	7 33 2	52 15 57	52 57	+480 M.										
11	S	7 18 1	28 4 55	+642 M.	+642 M.	11	Tu	6 53 1	57 14 36	+583 M.	+583 M.	11	F	7 50 3	21 18 11	18 11	+408 M.	11	S	8 36 3	40 13 29	40 29	+358 M.	11	S	8 36 3	40 13 29	40 29	+358 M.										
12	M	7 49 2	23 9 11	+520 M.	+520 M.	12	W	7 32 2	52 16 52	+461 M.	+461 M.	12	S	8 49 4	12 16 55	12 55	+286 M.	12	M	9 38 4	27 10 29	27 29	+337 M.	12	M	9 38 4	27 10 29	27 29	+337 M.										
13	Tu	8 23 3	17 12 46	+398 M.	+398 M.	13	Tu	8 18 3	47 18 7	+339 M.	+339 M.	13	S	9 50 5	14 53	14 53	+164 M.	13	W	10 41 5	11 7 5	11 5	+115 M.	13	W	10 41 5	11 7 5	11 5	+115 M.										
14	W	9 0 4	11 15 32	+275 M.	+275 M.	14	F	9 9 4	40 18 20	+217 M.	+217 M.	14	M	10 52 5	48 12 13	48 13	+42 M.	14	Tu	11 44 5	5 54 3 27 S	5 27 S	-07 M.	14	Tu	11 44 5	5 54 3 27 S	5 27 S	-07 M.										
15	Th	9 41 5	17 21	+153 M.	+153 M.	15	S	10 3 5	31 17 38	+95 M.	+95 M.	15	Tu	11 54 6	33 9 6	33 9	-709 M.	15	Th	12 47 6	36 0 20 N	0 20 N	-123 M.	15	Th	12 47 6	36 0 20 N	0 20 N	-123 M.										
16	F	10 26 5	18 12	+31 M.	+31 M.	16	S	11 2 6	21 16 6	-27 M.	-27 M.	16	W	12 56 7	16 5 38	16 38	-201 M.	16	F	13 54 8	7 18 4 7	7 47	-250 M.	16	F	13 54 8	7 18 4 7	7 47	-250 M.										
17	S	11 18 6	18 8	-91 M.	-91 M.	17	M	12 2 7	8 13 53	-48 M.	-48 M.	17	Th	13 59 7	58 1 57 S	1 57 S	-423 M.	17	S	15 59 8	47 11 12	47 12	-493 M.	17	S	15 59 8	47 11 12	47 12	-493 M.										
18	S	12 13 7	17 11	-213 M.	-213 M.	18	Tu	13 4 7	53 11 6	-270 M.	-270 M.	18	F	15 3 8	41 1 51 N	1 51 N	-444 M.	18	M	17 4 9	34 14 11	34 11	-614 M.	18	M	17 4 9	34 14 11	34 11	-614 M.										
19	Tu	13 11 8	25 15 28	-335 M.	-335 M.	19	W	14 6 8	37 7 53	-392 M.	-392 M.	19	S	16 5 9	24 5 36	5 36	-506 M.	19	Tu	18 10 10	16 16 34	16 34	-735 M.	19	Tu	18 10 10	16 16 34	16 34	-735 M.										
20	W	14 12 9	11 13 6	-456 M.	-456 M.	20	Th	15 8 9	20 4 22	-513 M.	-513 M.	20	M	17 10 10	8 9 11	8 9	-687 M.	20	W	19 12 11	17 18 8	17 8	-832 E.	20	W	19 12 11	17 18 8	17 8	-832 E.										
21	Th	15 12 9	56 10 12	-700 M.	-700 M.	21	F	16 11 10	2 0 39 S	-635 M.	-635 M.	21	Tu	18 16 10	55 12 27	12 27	-870 E.	21	Th	19 12 11	17 18 8	17 8	-832 E.	21	Th	19 12 11	17 18 8	17 8	-832 E.										
22	F	16 15 10	39 6 54	-822 M.	-822 M.	22	S	17 14 10	45 3 7 N	-878 E.	-878 E.	22	W	18 16 10	55 12 27	12 27	-870 E.	22	F	19 12 11	17 18 8	17 8	-832 E.	22	F	19 12 11	17 18 8	17 8	-832 E.										
23	S	17 17 11	22 3 20 S	+857 E.	+857 E.	23	M	18 18 11	28 6 48	-878 M.	-878 M.	23	Th	19 12 11	17 18 8	17 8	-852 E.	23	S	5 14 13	8 18 13	8 13	+701 E.	23	S	5 14 13	8 18 13	8 13	+701 E.										
24	S	6 21 12	47 4 6	+735 E.	+735 E.	24	Tu	5 43 13	0 13 17	+679 E.	+679 E.	24	F	6 26 14	21 18 35	21 35	+506 E.	24	M	7 30 15	0 13 59	0 59	+458 E.	24	M	7 30 15	0 13 59	0 59	+458 E.										
25	M	6 47 13	31 7 41	+613 E.	+613 E.	25	W	6 16 13	48 15 46	+558 E.	+558 E.	25	S	7 25 15	16 17 43	16 43	+385 E.	25	Tu	8 45 15	55 10 32	55 32	+337 E.	25	Tu	8 45 15	55 10 32	55 32	+337 E.										
26	Tu	7 11 14	16 10 59	+492 E.	+492 E.	26	Th	6 55 14	39 17 31	+436 E.	+436 E.	26	M	8 31 16	11 15 49	11 49	+264 E.	26	W	10 16 17	58 9 24	9 24	+216 E.	26	W	10 16 17	58 9 24	9 24	+216 E.										
27	W	7 42 15	21 52	+370 E.	+370 E.	27	F	7 40 15	32 18 24	+314 E.	+314 E.	27	Tu	9 42 17	5 12 59	5 59	+42 E.	27	Th	11 17 17	39 2 2 N	2 2 N	+94 E.	27	Th	11 17 17	39 2 2 N	2 2 N	+94 E.										
28	Th	8 15 15	51 16 8	+248 E.	+248 E.	28	S	8 33 16	25 18 17	+193 E.	+193 E.	28	W	10 56 17	58 9 24	9 24	+20 E.	28	Tu	12 33 18	31 2 6	2 6	+27 E.	28	Tu	12 33 18	31 2 6	2 6	+27 E.										
29	F	9 28 16	52 17 10	+176 E.	+176 E.	29	M	9 28 16	25 18 17	+193 E.	+193 E.	29	Th	11 54 19	8 11 59 N	8 59 N	-172 E.	29	W	12 33 18	31 2 6	2 6	+27 E.	29	Th	12 33 18	31 2 6	2 6	+27 E.										

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. DENNING.

Date.	Radiant.	Meteors.	Date.	Radiant.	Meteors.
	α δ			α δ	
Jan. 2-3...	230° +53°	Swift; long paths.	July-Sept. .	335° +73°	Swift; short.
3 ...	156° +41°	Swift.	July-Aug. .	339° -27°	Slow; long.
11 ...	220° +13°	Swift; streaks.	July-Oct. 8	30° +36°	Swift; streaks.
17 ...	295° +53°	Slow; bright.	Aug. 4-10	350° +49°	Swift.
17-23	159° +27°	Swift.	10-12	45° +57°	Swift; streaks.
25 ...	131° +32°	Swift.	10-Sept. 16	353° -11°	Rather slow.
29 ...	213° +52°	Very swift.	Aug. 15 ...	290° +53°	Swift; bright.
Feb. 5-10	75° +41°	Slow; bright.	21-25	291° +60°	Slow; bright.
15 ...	236° +11°	Swift; streaks.	25 ...	5° +11°	Slow; short.
15 ...	261° + 4°	Swift; streaks.	Aug.-Sept. .	346° ± 0°	Slow.
20 ...	181° +34°	Swift; bright.	Aug.-Oct. 2	74° +42°	Swift; streaks.
20 ...	263° +36°	Swift; streaks.	Sept. 3-8 .	353° +49°	Very swift.
Mar. 1-4...	166° + 4°	Slow; bright.	5-15	62° +37°	Swift; streaks.
14 ...	250° +54°	Swift.	6-17	106° +52°	Swift; streaks.
18 ...	316° +76°	Slow; bright.	15 ...	77° +57°	Swift; streaks.
24 ...	161° +58°	Swift.	21 ...	31° +19°	Slow.
27 ...	229° +32°	Swift; small.	27 ...	75° +15°	Swift; streaks.
28 ...	263° +62°	Rather swift.	Oct. 2 ...	230° +52°	Slow; bright.
Apr. 12-24	210° -10°	Slow; fireballs.	4 ...	133° +79°	Swift; streaks.
Ap.17-May 1	240° +47°	Small; short.	4 ...	310° +77°	Slowish.
Apr. 18-23	189° -31°	Slow; long.	8 ...	77° +31°	Swift; streaks.
20-21	261° +36°	Swift; bl. white.	8-14	45° +58°	Small; short.
20-22	271° +33°	Swift.	14 ...	133° +68°	Rather swift.
20-25	218° -31°	Slow; long paths.	16-23	89° + 8°	Swift; streaks.
30 ...	291° +59°	Rather slow.	18-20	92° +15°	Swift; streaks.
May 1-6 .	338° - 2°	Swift; streaks.	23 ...	99° +13°	Swift; streaks.
7 ...	246° + 3°	Slow; bright.	29 ...	109° +23°	Very swift.
11-18	231° +27°	Slow; small.	Nov. 1 ...	43° +22°	Slow; bright.
29 ...	264° +64°	Slowish.	2 ...	55° + 9°	Slow; bright.
30-Aug.	333° +27°	Swift; streaks.	10-12	133° +31°	Very swift; streaks.
May-June .	235° + 9°	Rather slow.	14-16	150° +22°	Swift; streaks.
May-July .	252° -21°	Slow.	16-28	154° +41°	Swift; streaks.
June 10 ...	261° + 5°	Very slow.	20-23	63° +23°	Slow; bright.
11-19	274° +69°	Rather swift.	17-23	25° +43°	Very slow; trains.
13 ...	310° +61°	Swift; streaks.	23-Dec. 12	189° +73°	Rather swift.
June-Sept. .	335° +57°	Swift; slow in Sep.	Nov. 30 ...	190° +58°	Swift; streaks.
June 26 ...	354° +39°	Swift; streaks.	Dec. 4 ...	162° +58°	Swift; streaks.
June-Aug. .	303° +24°	Swift.	6 ...	80° +23°	Slow; bright.
July 6-22	284° -13°	Very slow.	8 ...	145° + 7°	Swift; streaks.
15-31	23° +43°	Swift; streaks.	8 ...	208° +71°	Rather swift.
19 ...	315° +48°	Swift; short.	10-12	108° +33°	Swift; short.
July-Aug. .	269° +48°	Slow.	12 ...	119° +29°	Rather swift.
9-Sep. 18	323° -24°	Slow; long.	22 ...	194° +67°	Swift; streaks.
25-Sep. 15	48° +43°	Swift; streaks.	25 ...	98° +31°	Very slow.
July 28 ...	339° -11°	Slow; long.			

The radiant-points of the more brilliant showers are indicated by heavier type. The Leonids of November may be somewhat plentiful in 1904, November 15 A.M.

The Perseids, with max. on August 11, are visible for a considerable period and their radiant-point exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris:—

Date.	Radiant.	Date.	Radiant.	Date.	Radiant.
	α δ		α δ		α δ
July 19 ...	18°9' +50°5'	July 29 ...	29°3' +53°8'	Aug. 8 ...	41°5' +56°5'
21 ...	20°8' +51°1'	31 ...	31°6' +54°4'	10 ...	44°3' +56°9'
23 ...	22°8' +51°8'	Aug. 2 ...	33°9' +55°0'	12 ...	47°1' +57°3'
25 ...	24°9' +52°5'	4 ...	36°4' +55°5'	14 ...	50°0' +57°7'
27 ...	27°1' +53°2'	6 ...	38°9' +56°0'	16 ...	52°9' +58°0'

Mars (♂).

Venus (♀).

Mercury (☿).

1904.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Trans- sits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Trans- sits.	Sets.
	h m s	° ' S.	''	h m	h m	h m	h m s	° ' N.	''	h m	h m	h m
Jan. 1...	20 6 9	18 10	68	21 23	1 27	5 33	15 36 15	16 40 S.	180	16 23	20 58	1 33
Jan. 1...	20 16 22	18 20	90	21 33	0 58	5 28	16 24 15	17 14 S.	168	16 47	21 7	1 27
Jan. 1...	19 28 42	18 20	100	18 59	23 23	3 47	17 14 16	21 4	156	17 9	21 17	1 25
Jan. 1...	19 14 7	19 59	82	20 22	25 35	2 50	18 5 54	21 58	148	17 27	21 30	1 33
Feb. 10...	19 45 31	20 43	68	18 29	22 39	3 39	18 57 55	21 50	140	17 38	21 42	1 46
Feb. 10...	20 35 6	19 16	58	18 24	22 32	3 39	19 49 34	22 39	172	17 44	21 55	2 16
Mar. 20...	21 35 4	16 32	54	18 26	23 23	3 40	20 49 34	18 39	172	17 44	21 55	2 16
Mar. 20...	22 38 11	11 0	50	18 18	23 26	3 44	21 30 27	15 25	152	17 31	22 16	2 59
Mar. 20...	23 45 7	3 33 S.	50	18 18	23 34	3 40	22 18 24	11 38	118	17 31	22 16	2 59
Mar. 20...	23 45 7	5 32 N.	52	17 53	23 35	3 43	23 50 59	2 40 S.	114	16 48	22 38	3 59
Apr. 10...	2 9 21	14 59	52	17 35	0 56	8 77	23 50 59	2 8 N.	108	16 29	22 44	4 59
Apr. 20...	3 46 20	21 43	74	17 21	1 14	9 7	23 50 59	6 54	104	16 10	22 49	5 28
Apr. 20...	3 33 13	21 43	74	16 57	1 1	9 5	23 50 59	6 54	104	15 53	23 56	5 59
May 10...	3 25 30	19 6	118	16 29	0 14	7 59	2 7	11 25	100	15 39	23 5	6 31
May 20...	3 7 5	15 6	118	16 29	0 14	7 59	2 7	11 25	100	15 39	23 5	6 31
May 20...	3 6 41	13 34	100	15 25	23 11	6 32	2 54 36	15 31	100	15 28	23 5	7 2
June 9...	3 32 46	15 32	80	14 57	22 34	5 48	3 43 51	18 58	100	15 24	23 26	7 30
June 9...	4 23 22	19 29	64	14 45	22 33	5 49	4 34 57	21 36	98	15 24	23 40	7 56
June 9...	5 38 57	23 12	54	14 58	23 15	7 34	5 21 8	23 14	98	15 52	0 7	8 22
July 9...	7 11 8	23 30	50	15 41	0 3	8 25	5 14 40	23 5	96	16 18	0 20	8 22
July 9...	8 39 23	20 17	52	16 57	0 52	8 40	8 7 19	21 18	98	16 48	0 32	8 16
July 9...	9 50 55	14 17	56	18 5	1 41	8 43	8 58 22	18 30	98	17 19	0 41	8 1
Aug. 8...	10 46 55	7 37	62	18 58	1 41	8 43	9 47 33	14 50	98	17 51	0 40	7 47
Aug. 8...	11 29 12	1 25 N.	72	19 23	1 44	7 55	10 34 57	10 29	100	18 23	0 56	7 20
Aug. 8...	11 54 52	3 8 S.	84	19 43	7 17	7 17	11 28 55	5 41	102	18 23	1 1	6 48
Sept. 7...	11 53 50	4 0 S.	100	19 6	0 49	6 32	12 6 3	5 77 N.	103	19 25	1 7	6 48
Sept. 7...	11 53 50	4 0 S.	102	17 27	23 33	5 39	12 5 3	4 31 S.	103	19 25	1 7	6 48
Sept. 7...	11 13 47	5 20 N.	80	16 19	22 49	5 19	13 30 53	9 30	108	19 58	1 13	6 28
Oct. 7...	11 54 6	3 59 S.	60	16 32	22 53	5 14	14 23 57	14 9	112	20 30	1 30	6 10
Oct. 7...	12 54 37	11 7	50	17 31	23 15	5 49	15 14 55	18 14	116	21 4	1 20	5 50
Oct. 7...	13 56 57	11 7	48	18 31	23 38	4 32	16 54 32	23 53	120	21 36	1 41	5 46
Nov. 6...	14 59 9	7 19	46	19 31	23 59	4 32	17 48 9	25 4	130	22 48	2 8	5 48
Nov. 6...	15 59 9	22 5	50	20 20	0 22	4 44	18 42 4	25 4	134	22 41	2 22	6 7
Dec. 1...	17 41 21	24 3	50	21 6	0 48	4 33	19 35 9	23 45	142	22 45	2 36	6 54
Dec. 1...	18 19 11	24 37	58	21 37	1 13	4 49	20 26 24	21 21	150	22 42	2 48	7 25
Dec. 16...	19 15	24 13	70	21 37	1 44	5 11	20 26 24	21 21	150	22 42	2 48	7 25
Dec. 26...	19 6 52	21 32 S.	92	20 44	0 48	4 52	21 15 9	17 59 S.	160	22 29	2 57	7 25

Mercury.

	d h	d h	d h
Gr. elong.	W.Feb. 9 21;	June 8 8;	Oct. 1 7
"	E. Apr. 21 8;	Aug. 19 16;	Dec. 13 21
Sup. conj.	Mar. 26 9;	July 9 11;	Oct. 30 22
Inf. conj.	Jan. 17 0;	May 12 23;	Sept. 15 14
			Dec. 31 4

Venus.

Sup. conj. July 7 20 d h

Superior Planets.

Opposition.	Quadrature.	Stationary.
d h	d h	d h
Oct. 18 11	July 21 15	Aug. 20 4; Dec. 15 23
Aug. 10 7	May 11 8; Nov. 7 0	June 1 3; Oct. 18 22
June 19 5	Mar. 20 10; Sept. 18 23	Apr. 3 21; Sept. 4 11

Jupiter (♃).

Saturn (♄).

1904.	R.A. Noon.			Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.			Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h	m	s	°	"	h	m	h	m	s	°	"	h	m	h	m
Jan. 1	23	17	10	5 56 S.	35.8	23	5	4 38	10	11	20	42 41	18 56 S.	14.2	21	42
21	23	30	5	4 30	33.0	21	52	3 32	9	12	20	52 3	18 21	14.2	20	29
Feb. 10	23	45	19	2 49	31.8	20	39	2 28	8	17	21	1 43	17 42	14.0	19	14
Mar. 1	0	2	4	0 58 S.	31.0	19	28	1 26	7	24	21	11 5	17 4	14.2	18	0
21	0	19	38	0 56 N.	30.6	18	17	0 25	6	33	21	19 32	16 28	14.4	16	46
Apr. 10	0	37	24	2 51	30.8	17	4	23 21	5	38	21	26 33	15 58	14.8	15	32
20	0	54	49	4 40	31.2	15	53	22 20	4	47	21	31 39	15 37	15.4	14	16
May 30	1	11	18	6 20	32.0	14	43	21 18	3	53	21	34 26	15 27	15.8	12	59
June 9	1	26	12	7 47	33.4	13	32	20 14	2	56	21	34 42	15 29	16.4	11	40
29	1	38	48	8 56	35.2	12	20	19 8	1	56	21	32 29	15 44	16.8	10	17
July 19	1	48	13	9 45	37.2	11	5	17 58	0	51	21	28 10	16 7	17.2	9	0
Aug. 8	1	53	35	10 10	39.8	9	50	16 45	23	40	21	22 34	16 36	17.4	7	39
28	1	54	8	10 8	42.2	8	31	15 26	22	21	21	16 46	17 4	17.2	6	18
Sept. 17	1	49	43	9 39	44.4	7	11	14 3	20	55	21	11 59	17 26	17.0	4	56
Oct. 7	1	41	19	8 49	45.8	5	48	12 36	19	24	21	9 10	17 38	16.4	3	36
27	1	31	15	7 52	45.8	4	26	11 8	17	50	21	8 54	17 38	16.0	2	17
Nov. 16	1	22	28	7 4	44.4	3	2	9 40	16	18	21	11 21	17 26	15.4	1	0
Dec. 6	1	17	28	6 40	42.4	1	41	8 17	14	53	21	16 16	17 4	15.0	23	44
26	1	17	29	6 46 N.	39.6	0	21	6 58	13	35	21	23 13	16 31 S.	14.6	22	29

Uranus (♅).

Neptune (♆).

Jan. 1	17 45 17	23 34 S.	3.4	19 12	23 3	2 54	6 19 3	22 17 N.	2.7	7 38	11 38	15 3
31	17 52 29	23 36	3.4	17 21	21 12	1 3	6 15 43	22 20	2.7	5 38	9 37	13 3
Mar. 1	17 57 39	23 37	3.5	15 28	19 19	23 10	6 13 47	22 21	2.6	3 38	7 37	11 3
31	17 59 46	23 38	3.6	13 32	17 23	21 14	6 13 55	22 23	2.6	1 40	5 39	9 3
Apr. 30	17 58 34	23 38	3.6	11 33	15 24	19 15	6 16 9	22 23	2.6	23 45	3 44	7 4
May 30	17 54 36	23 39	3.7	9 32	13 22	17 12	6 20 1	22 22	2.5	21 50	1 49	5 4
June 29	17 49 23	23 38	3.7	7 28	11 19	15 10	6 24 43	22 20	2.5	39 53	23 52	3 5
July 29	17 44 46	23 37	3.6	5 25	9 16	13 7	6 29 22	22 17	2.5	18 0	21 59	1 5
Aug. 28	17 42 26	23 36	3.5	3 25	7 16	11 7	6 33 5	22 14	2.6	16 50	20 5	0
Sept. 27	17 43 17	23 36	3.5	1 28	5 19	9 10	6 35 7	22 12	2.6	14 9	19 8	9 22
Oct. 27	17 47 19	23 37	3.4	23 34	3 25	7 16	6 35 4	22 11	2.6	12 11	16 11	20 1
Nov. 26	17 53 49	23 39	3.3	21 44	1 34	5 24	6 32 59	22 12	2.7	10 11	14 10	18
Dec. 26	18 1 34	23 40 S.	3.3	19 49	23 40	3 31	6 29 36	22 15 N.	2.7	8 9	12 9	16

Ceres.

Pallas.

1904.	R.A. Noon.		Dec. Noon.	Transits.	1904.	R.A. Noon.		Dec. Noon.	Transits.
	h	m	°	h		h	m	°	h
May 10 ...	17	19	20 2 S.	14 5	Apr. 16 ...	16	35	20 6 N.	14 56
22 ...	17	10	20 28	13 9	28 ...	16	29	22 42	14 3
June 3 ...	16	59	20 54	12 10	May 10 ...	16	21	24 43	13 7
15 ...	16	47	21 19	11 12	22 ...	16	11	25 59	12 10
27 ...	16	37	21 44 S.	10 15	June 3 ...	16	1	26 25 N.	11 13

Juno.

July 9 ...	21 15	2 0 S.	14 5
21 ...	21 8	2 38	13 10
Aug. 2 ...	20 59	3 44	12 14
14 ...	20 48	5 15	11 16
26 ...	20 39	6 59 S.	10 20

The times of rising and setting correspond to the adjacent transit, irrespective of the day given in the first column.

ECLIPSES IN 1904.

In the year 1904 there will be two Eclipses of the Sun.

I.—*An Annular Eclipse of the Sun, 1904, March 16.*

Invisible at Greenwich. Will be visible as an Annular Eclipse from East Africa, the Indian Ocean, and further India only. Central Eclipse begins generally March 16^d 15^h 44^m.2 in longitude 35° 51' East, latitude 10° 15' South, and ends at March 16^d 19^h 37^m.4 in longitude 157° 6' East and latitude 25° 13' N.

A Partial Eclipse will be visible at the following places (Local Mean Times are given):—

	Mauritius.			Madras.			Hong Kong.		
	d	h	m	d	h	m	d	h	m
Eclipse begins	16	18	29.8	16	20	53.2	17	0	47.2
Greatest phase	16	19	40.5	16	22	32.0	17	2	35.7
Eclipse ends	16	21	4.3	17	0	23.5	17	4	8.1
Magnitude			0.654			0.568			0.827

II.—*A Total Eclipse of the Sun, 1904, September 9.*

Invisible at Greenwich. The Line of Totality lies wholly in the Pacific Ocean. The only land over which the shadow passes when the Sun is at an altitude sufficient for useful observation is the group of Marshall Islands, of which the island of Aurb, in longitude about 172° East and latitude 8° N., is one. The Sun's altitude here will be about 9° and duration of totality 3^m 18^s. The duration of totality reaches 6^m 24^s in longitude 134° 23' West, latitude 3° 49' S. Fanning Island, 159° 40' W., 4° North latitude (which is the island where the Pacific cables come to land in mid-ocean), and Palmyra Island are near the Line of Totality.

Central Eclipse begins generally September 9^d 7^h 3^m.2 in longitude 162° 48' East of Greenwich, latitude 7° 52' North, and ends September 9^d 10^h 25^m.6 in longitude 69° 43' West, latitude 26° 39' S.

A partial eclipse will be visible at Honolulu. Eclipse begins Sept. 9^d 6^h 45^m. Ends 8^h 50^m G.M.T. Magnitude about 0.5.

OCCULTATIONS, 1904. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap.		Reap.		Date.	Star.	Mag.	Disap.		Reap.
			M. T.	P.	M. T.	P.				M. T.	P.	M. T.
Jan.			h m	°	h m	°	June			h m	°	h m
1	111 Tauri	5.2	6 8	129	6 49	214	16	29 Cancri	5.9	8 28	132	9 11
1	130 Tauri	5.5	17 0	83	17 50	284	24	θ Libræ	4.3	11 55	162	12 30
2	D.M. + 17° 1203	6.5	3 26†	139	3 55*	215	27	D.M. - 19° 5134	6.5	14 45	135	15 20
2	D.M. + 17° 1214	6.5	4 16	40	4 49	314						
2	B.A.C. 2116	6.5	8 31	62	9 31	296	July					
2	26 Geminorum	5.1	13 34	66	14 32	308	7	85 Ceti	6.3	13 57	98	14 50
5	o Leonis	3.8	10 13	129	11 9	259	9	71 Tauri	4.6	13 24†	145	13 40
5	B.A.C. 3398	5.9	18 19	50	18 49	351	9	θ ¹ Tauri	3.9	14 5	79	14 51
5	B.A.C. 3407	6.4	19 18	154	19 59	245	9	θ ² Tauri	3.6	14 12	100	15 0
7	D.M. + 0° 2843	6.5	18 38	148	19 36	259	9	D.M. + 15° 633	6.5	14 25	37	15 0
10	2 Libræ	6.3	15 43	76	16 40	327	9	B.A.C. 1391	4.9	15 0	48	15 40
11	o ¹ Libræ	6.0	19 36	174	20 7	220	9	α Tauri	1.1	17 31	39	18 20
20	B.A.C. 7774	6.2	3 58	11	4 53	202	10	111 Tauri	5.2	13 45†	56	14 20
25	ξ Arietis	5.5	12 39	54	13 28†	284	10	117 Tauri	6.5	15 2	110	15 40
26	D.M. + 12° 436	5.9	7 19	25	8 11	302	27	D.M. - 15° 5848	6.0	13 25	27	14 20
26	B.A.C. 987	6.3	10 9	137	10 45	201	30	96 Tauri	5.7	12 47	83	14 0
28	B.A.C. 1526	5.8	6 16	70	7 28	271						
29	D.M. + 17° 1203	6.5	16 26	124	17 9	246	Aug.					
30	λ Geminorum	3.6	15 6	106	16 3	275	5	D.M. + 14° 657	6.5	14 0	68	15 0
Feb.							27	20 Piscium	5.7	10 0	54	11 10
24	α Tauri	1.1	5 57	73	7 15	268	30	64 Ceti	5.7	13 37	85	14 50
29	o Leonis	3.8	8 53	146	9 46	249	30	ξ ¹ Ceti	4.5	14 51	62	16 10
29	B.A.C. 3398	5.9	16 27	61	17 3	336	Sept.					
29	B.A.C. 3407	6.4	17 29	173	17 50	222	2	89 Tauri	6.5	11 19	83	12 10
Mar.							2	σ ² Tauri	4.8	11 55	125	12 30
4	m Virginis	5.3	13 13	110	14 25	296	3	B.A.C. 1728	5.5	11 12	117	11 50
5	B.A.C. 4828	6.0	13 7	87	14 12	315	19	(1st *)				
22	θ ¹ Tauri	3.9	10 0	131	10 41	223	23	D.M. - 16° 5609	6.0	11 30	5	11 50
22	75 Tauri	5.3	10 4	30	10 35	324	27	B.A.C. 8184	6.3	6 0	114	6 40
22	D.M. + 15° 633	6.5	10 4	84	11 1	270	29	85 Ceti	6.3	10 47	56	11 50
22	B.A.C. 1391	4.9	10 46	80	11 39	274	29	48 Tauri	6.4	7 32†	47	8 10
23	111 Tauri	5.2	10 6	70	11 1	293	29	γ Tauri	3.9	9 17	39	10 0
23	117 Tauri	6.5	11 40	95	12 33	268	29	71 Tauri	4.6	12 33	137	13 0
24	D.M. + 17° 1203	6.5	8 56	136	9 48	235	29	θ ¹ Tauri	3.9	13 40	80	14 50
25	λ Geminorum	3.6	9 2	108	10 11	272	29	θ ² Tauri	3.6	13 50	103	15 0
Apr.							29	D.M. + 15° 633	6.5	14 8	41	15 10
3	49 Libræ	5.6	11 4	94	12 9	297	29	B.A.C. 1391	4.9	15 1	54	16 10
5	B.A.C. 6060	6.5	14 39	34	15 17	336	30	α Tauri	1.1	18 42	17	19 10
8	D.M. - 16° 5609	6.0	14 24†	126	15 15	221	30	111 Tauri	5.2	14 54	81	16 10
22	B.A.C. 2737	6.3	13 20	113	14 8†	270	30	117 Tauri	6.5	17 21	143	18 0
24	B.A.C. 3398	5.9	9 25	118	10 33	284	Oct.					
28	m Virginis	5.3	10 6	103	11 18	303	1	D.M. + 17° 1214	6.5	15 41	83	16 50
29	B.A.C. 4828	6.0	9 40	75	10 37	327	17	D.M. - 15° 5848	6.0	8 16	11	8 50
May							20	96 Aquarii	5.7	7 49	66	9 10
3	Y Sagittarii	var	10 33†	55	11 23	314	24	85 Ceti	6.3	18 52	116	19 30
4	o ² Sagittarii	6.1	14 48	118	15 57	231	26	D.M. + 14° 657	6.5	9 15	84	10 20
6	D.M. - 15° 5848	6.0	13 19	151	13 42	190	29	γ Tauri	3.9	16 48	142	17 20
7	λ Capricorni	5.4	13 19†	78	14 26	257		D.M. + 17° 1479	6.2	15 22	29	15 50
17	130 Tauri	5.5	7 27	76	8 20	290	Nov.					
21	o Leonis	3.8	9 1	177	9 26	223	2	37 Sextanti	6.2	14 13	123	15 0
26	2 Libræ	6.3	13 30	104	14 35	286	17	20 Piscium	5.7	6 34	19	7 30
26	B.A.C. 4772	6.5	14 10	76	15 4†	311	20	64 Ceti	5.7	9 13	98	10 20
27	o ² Libræ	6.8	15 43†	97	16 42†	280	20	ξ ¹ Ceti	4.5	10 24	75	11 40

† Star below the horizon.

* Star rising.

‡ Star setting.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., &c. from the bottom of the Moon's inverted image towards the right.

Date.	Star.	Mag.	Disap.		Reap.		Date.	Star.	Mag.	Disap.		Reap.
			M. T.	P.	M. T.	P.				M. T.	P.	M. T.
Nov.			h m	°	h m	°	Dec.			h m	°	h m
22	D.M.+14° 657.	6.5	18 17	132	18 54	218	20	75 Tauri.....	5.3	11 16	42	12 18
23	89 Tauri	6.5	5 58	152	6 12	183	20	D.M.+15° 633.	6.5	11 24	93	12 39
23	B.A.C. 1526 ...	5.8	16 12	68	17 15	287	20	θ ¹ Tauri	3.9	11 25	145	11 58
24	130 Tauri	5.5	13 14	150	13 49	202	20	B.A.C. 1391 ...	4.9	12 21	100	13 31
25	B.A.O. 2116 ...	6.5	7 11	97	8 3	255	20	α Tauri	1.1	15 19	57	16 12
26	D.M.+17° 1596	5.6	8 22	63	9 11	298	21	115 Tauri	5.4	13 15	24	13 48
Dec.							22	D.M.+17° 1182	6.5	7 27	115	8 22
1	13 Virginis	6.3	13 9†	99	14 3	299	22	B.A.O. 2116 ...	6.5	17 0	142	17 41
1	η Virginis	4.0	13 56	32	14 8	6	23	D.M.+17° 1596	5.6	17 18	98	18 19
13	λ Aquarii	3.9	10 19	98	11 12†	222	24	D.M.+16° 1679	6.5	10 49	119	11 54
14	B.A.C. 8184 ...	6.3	4 51	0	5 26	311	26	A Leonis	4.6	9 2	23	9 13
18	85 Ceti	6.3	9 36	73	10 55	248	28	13 Virginis	6.3	20 8	87	21 8
20	48 Tauri	6.4	4 1	84	4 58	247	29	κ Virginis	5.9	12 47	68	13 30
20	γ Tauri	3.9	6 1	90	7 4	239	30	B.A.C. 4680 ...	6.5	18 28	90	19 37

† Star below the horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
			h m	°				h m	°
Jan. 1	115 Tauri	5.4	7 53	352	May 3	B.A.C. 6287 ...	5.7	16 11	357
10	B.A.C. 4772 ...	6.5	16 48	22	4	ρ ¹ Sagittarii ...	3.9	15 25	355
29	D.M.+17° 1182	6.5	14 58	6	June 25	B.A.C. 5579 ...	5.1	7 40	13
29	D.M.+17° 1214	6.5	17 34	5	29	D.M.—16° 5609	6.0	16 43	343
31	1 Cancri	6.0	4 29	4	July 9	75 Tauri	5.3	14 36	348
31	29 Cancri	5.9	18 35	13	Sept. 4	B.A.C. 2116 ...	6.5	12 55	356
Feb. 8	49 Libræ	5.6	19 53	11	Oct. 1	D.M.+17° 1182	6.5	12 18	354
25	130 Tauri	5.5	13 37	3	14	Y Sagittarii ...	var.	7 9	356
Mar. 8	D.M.—18° 4516	6.3	16 35	7	Nov. 23	α Tauri	1.1	5 20	348
24	D.M.+17° 1214	6.5	10 25	6	30	89 Leonis	5.7	18 46	25
26	29 Cancri	5.9	14 42	14	Dec. 28	η Virginis	4.0	21 9	22
Apr. 1	96 Virginis	6.5	12 1	22	30	95 Virginis	5.7	20 21	22
22	1 Cancri	6.0	7 9	13					

South Near Approaches.

					h m										h m				
Jan.	5	π Leonis		4.8	20	36	198	Sept.	2	σ ¹ Tauri		5.1	12	9	168				
	13	29 Ophiuchi	...	6.5	17	28	191		3	122 Tauri		5.4	13	34	171				
	29	D.M.+17° 1191	...	6.5	15	50	185		24	78 Aquarii		6.3	13	43	157				
	31	B.A.C. 2737	...	6.3	10	52	192		29	85 Tauri		6.5	16	17	170				
Feb.	2	43 Leonis		6.5	15	56	202	Oct.	1	D.M.+17° 1203		6.5	14	55	176				
	8	θ Libræ		4.3	16	5	197		26	48 Tauri		6.4	14	36	169				
Mar.	22	θ ² Tauri		3.6	10	27	177		30	1 Cancri		6.0	14	34	186				
	22	85 Tauri		6.5	11	43	177	Nov.	13	B.A.C. 7145	...	6.0	7	16	163				
	24	D.M.+17° 1191	...	6.5	8	0	184		25	26 Geminorum		5.1	12	27	179				
	26	B.A.C. 2737	...	6.3	6	15	190	Dec.	3	κ Virginis		4.3	15	55	200				
	28	43 Leonis		6.5	14	5	201		25	10 Ceti		6.4	12	40	162				
Apr.	18	48 Tauri		6.4	10	8	176		20	θ ² Tauri		3.6	11	49	170				
	21	D.M.+16° 1363	...	6.5	9	16	190		20	89 Tauri		6.5	16	43	176				
May	29	29 Ophiuchi	...	6.5	9	16	192		21	111 Tauri		5.2	11	51	175				
June	28	B.A.C. 6710	...	6.0	15	54	167		22	D.M.+17° 1214		6.5	11	35	178				

JUPITER'S SATELLITES, 1904.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; o, Occ. dis.; O, Occ. re.; t, Tr. Ing.; T, Tr. Egr.

January.	Jan. (con.).	Jan. (con.).	Feb. (con.).	Feb. (con.).	Feb. (con.).
h m	d h m	d h m	d h m	d h m	d h m
9 53 i. t.	13 19 23 i. t.	24 19 2 iii. e.	4 21 8 ii. T.	16 23 26 iv. E.	29 17 51 i. o.
12 11 i. T.	20 7 iii. t.	21 46 iii. E.	22 45 i. o.	17 5 8 ii. o.	18 59 ii. T.
22 46 ii. o.	21 41 i. T.	25 2 13 ii. t.	5 1 50 i. E.	8 17 i. o.	20 32 i. E.
3 58 ii. E.	21 55 iv. o.	4 57 ii. T.	19 57 i. t.	9 7 ii. E.	
7 13 i. o.	23 23 iii. T.	7 43 i. o.	22 14 i. T.	11 11 i. E.	April.
10 43 i. E.	14 1 34 iv. O.	10 57 i. E.	6 12 50 ii. o.	18 5 31 i. t.	23 1 34 ii. t.
1 39 iii. o.	9 30 iv. e.	26 4 54 i. t.	17 9 ii. E.	7 48 i. T.	4 11 ii. T.
4 23 i. t.	10 3 ii. t.	7 12 i. T.	17 15 i. o.	18 22 iii. t.	11 22 iii. t.
4 56 iii. O.	11 19 iv. E.	20 33 ii. o.	20 19 i. E.	21 30 iii. T.	14 4 iii. T.
6 41 i. T.	12 48 ii. T.	27 1 11 ii. E.	7 14 27 i. t.	19 0 3 ii. t.	18 26 i. t.
6 54 iii. e.	16 42 i. o.	2 13 i. o.	16 45 i. T.	2 45 ii. T.	20 41 i. T.
9 41 iii. E.	20 4 i. E.	5 26 i. E.	23 36 iii. o.	2 48 i. o.	24 15 10 i. e.
17 56 ii. t.	15 13 53 i. t.	23 25 i. t.	8 2 47 iii. O.	5 40 i. E.	17 52 i. O.
20 41 ii. T.	16 11 i. T.	28 1 42 i. T.	3 7 iii. e.	20 0 1 i. t.	19 37 ii. e.
1 43 i. o.	16 4 20 ii. o.	4 55 iii. t.	5 47 iii. E.	2 19 i. T.	23 12 ii. O.
5 11 i. E.	9 14 ii. E.	8 8 iii. T.	6 17 iv. t.	18 34 ii. o.	25 12 56 i. t.
22 53 i. t.	11 12 i. o.	15 37 ii. t.	7 49 ii. t.	21 18 i. o.	15 12 i. T.
1 11 i. T.	14 33 i. E.	18 21 ii. T.	9 36 iv. T.	22 26 ii. E.	26 9 38 i. e.
12 9 ii. o.	17 8 23 i. t.	20 43 i. o.	10 32 ii. T.	21 0 9 i. E.	12 23 i. O.
13 36 iv. t.	10 19 iii. o.	23 55 i. E.	11 45 i. o.	18 32 i. t.	14 59 ii. t.
17 16 ii. E.	10 41 i. T.	29 17 55 i. t.	14 48 i. E.	20 49 i. T.	17 35 ii. T.
17 18 iv. T.	13 34 iii. O.	20 13 i. T.	9 8 58 i. t.	22 8 36 iii. o.	23 29 iii. e.
20 13 i. o.	15 0 iii. e.	30 9 58 ii. o.	11 15 i. T.	13 28 ii. t.	27 4 12 iii. O.
23 40 i. E.	17 44 iii. E.	14 31 ii. E.	10 2 16 ii. o.	13 49 iii. E.	7 27 i. t.
15 47 iii. t.	23 26 ii. t.	15 14 i. o.	6 16 i. o.	15 49 i. o.	9 42 i. T.
17 23 i. t.	18 2 11 ii. T.	18 13 iv. o.	6 28 ii. E.	16 10 ii. T.	28 4 7 i. e.
19 4 iii. T.	5 42 i. o.	18 24 i. E.	9 16 i. E.	18 37 i. E.	6 53 i. O.
19 41 i. T.	9 2 i. E.	21 41 iv. O.	11 3 28 i. t.	23 13 3 i. t.	8 56 ii. e.
7 18 ii. t.	19 2 53 i. t.	31 3 56 iv. e.	5 46 i. T.	15 20 i. T.	12 38 ii. O.
10 3 ii. T.	5 11 i. T.	5 24 iv. E.	13 52 iii. t.	24 8 1 ii. o.	29 1 57 i. t.
14 43 i. o.	17 44 ii. o.	12 25 i. t.	17 2 iii. T.	10 19 i. o.	4 12 i. T.
18 9 i. E.	22 33 ii. E.	14 43 i. T.	21 14 ii. t.	11 45 ii. E.	22 35 i. e.
11 53 i. t.	20 0 13 i. o.	19 8 iii. o.	23 57 ii. T.	13 6 i. E.	30 1 23 i. O.
14 11 i. T.	3 31 i. E.	22 21 iii. O.	12 0 46 i. o.	25 3 14 iv. t.	4 24 ii. t.
1 32 ii. o.	21 23 i. t.	23 5 iii. e.	3 45 i. E.	6 15 iv. T.	6 59 ii. T.
6 36 ii. E.	23 41 i. T.		21 59 i. t.	7 33 i. t.	15 54 iii. t.
9 13 i. o.	21 0 29 iii. t.	February.	13 0 16 i. T.	9 51 i. T.	18 32 iii. T.
12 38 i. E.	3 44 iii. T.		15 42 ii. o.	22 54 iii. t.	20 28 i. t.
5 57 iii. o.	12 49 ii. t.	1 1 47 iii. E.	19 17 i. o.	26 1 59 iii. T.	22 43 i. T.
6 23 i. t.	15 34 ii. T.	5 1 ii. t.	19 48 ii. E.	2 53 ii. t.	
8 41 i. T.	18 43 i. o.	7 44 i. o.	22 14 i. E.	4 50 i. o.	May.
9 14 iii. O.	22 0 i. E.	9 44 i. o.	14 16 30 i. t.	5 34 ii. T.	1 17 4 i. e.
10 57 iii. e.	22 9 40 iv. t.	12 52 i. E.	18 47 i. T.	7 35 i. E.	19 53 i. O.
13 44 iii. E.	13 14 iv. T.	2 6 56 i. t.	15 4 5 iii. o.	27 2 4 i. t.	22 15 ii. e.
20 40 ii. t.	15 54 i. t.	9 13 i. T.	9 48 iii. E.	4 21 i. T.	2 2 4 ii. C.
23 25 ii. T.	18 11 i. T.	23 24 ii. o.	10 39 ii. t.	21 27 ii. o.	14 58 i. t.
3 43 i. o.	23 7 8 ii. o.	3 3 50 ii. E.	13 21 ii. T.	23 21 i. o.	17 13 i. T.
7 7 i. E.	11 53 ii. E.	4 14 i. o.	13 47 i. o.	28 1 5 ii. E.	3 11 32 i. e.
0 53 i. t.	13 13 i. o.	7 21 i. E.	16 42 i. E.	2 4 i. E.	14 24 i. O.
3 11 i. T.	16 29 i. E.	1 26 i. t.	16 11 0 i. t.	20 35 i. t.	17 48 ii. t.
14 56 ii. o.	24 10 24 i. t.	3 44 i. T.	13 17 i. T.	22 52 i. T.	20 24 ii. T.
19 55 ii. E.	12 42 i. T.	9 23 iii. t.	15 0 iv. o.	29 13 8 iii. o.	4 3 30 iii. e.
22 12 i. o.	14 43 iii. o.	12 34 iii. T.	18 10 iv. O.	16 18 ii. t.	5 54 iii. t.
1 36 i. E.	17 57 iii. O.	18 25 ii. t.	22 26 iv. e.	17 50 iii. E.	6 4 iii. e.

May (con.).			May (con.).			June (con.).			June (con.).			July (con.).			July (con.).		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
4	8	39 iii. O.	19	12	55 i. O.	2	22	1 ii. e.	18	11	54 i. e.	1	8	26 ii. e.	15	1	2
9	28	i. t.	16	47	ii. e.	3	2	43 ii. O.	15	20	i. O.	10	54	ii. E.	1	29	
11	43	i. T.	21	8	ii. O.	12	1	i. t.	23	52	ii. t.	11	6	ii. o.	3	21	
5	6	i. e.	20	8	i. t.	14	15	i. T.	19	2	23 ii. T.	13	36	ii. O.	4	36	
8	54	i. O.	10	15	i. T.	4	8	6 i. e.	10	28	i. t.	19	53	i. t.	13	37	
11	33	ii. e.	21	4	18 i. e.	11	23	i. O.	12	41	i. T.	22	6	i. T.	16	4	
15	29	ii. O.	7	25	i. O.	18	22	ii. t.	22	54	iii. t.	2	15	42 i. e.	16	24	
3	59	i. t.	12	49	ii. t.	20	55	ii. T.	20	1	2 iii. T.	19	14	i. O.	18	52	
6	14	i. T.	15	23	ii. T.	5	6	30 i. t.	6	22	i. e.	3	5	17 ii. t.	23	44	
7	0	29 i. e.	22	2	31 i. t.	8	44	i. T.	9	49	i. O.	7	47	ii. T.	16	1	57
3	24	i. O.	4	45	i. T.	14	13	iii. t.	16	32	ii. e.	14	22	i. t.	19	30	
7	13	ii. t.	5	23	iii. t.	16	30	iii. T.	19	0	ii. E.	16	35	i. T.	23	5	
9	48	ii. T.	7	49	iii. T.	6	2	34 i. e.	19	3	ii. o.	4	7	24 iii. t.	17	10	37
20	25	iii. t.	22	46	i. e.	5	53	i. O.	21	34	ii. O.	9	23	iii. T.	13	4	
22	29	i. t.	23	1	54 i. O.	11	20	ii. e.	21	4	57 i. t.	10	11	i. e.	18	13	
22	59	iii. T.	6	6	ii. e.	16	6	ii. O.	7	11	i. T.	13	43	i. O.	20	25	
8	0	44 i. T.	10	32	ii. O.	7	1	0 i. t.	22	0	51 i. e.	21	44	ii. e.	18	13	59
18	58	i. e.	21	1	i. t.	3	14	i. T.	4	19	i. O.	5	0	11 ii. E.	15	41	
21	54	i. O.	23	15	i. T.	21	3	i. e.	13	14	ii. t.	0	26	ii. o.	17	30	
9	0	52 ii. e.	24	17	15 i. e.	8	0	23 i. O.	15	44	ii. T.	2	55	ii. O.	17	33	
4	54	ii. O.	20	24	i. O.	7	45	ii. t.	23	27	j. t.	8	51	i. t.	19	2	55
16	59	i. t.	25	2	12 ii. t.	10	17	ii. T.	23	1	40 i. T.	11	4	i. T.	5	22	
19	14	i. T.	4	46	ii. T.	19	30	i. t.	7	40	iii. e.	6	4	39 i. e.	5	42	
10	13	27 i. e.	15	31	i. t.	21	43	i. T.	9	53	iii. E.	8	12	i. O.	8	9	
16	24	i. O.	15	34	iii. e.	23	37	iii. e.	12	56	iii. o.	18	37	ii. t.	12	41	
20	37	ii. t.	17	44	i. T.	9	1	53 iii. E.	15	1	iii. O.	21	7	ii. T.	14	54	
23	12	ii. T.	17	53	iii. E.	4	17	iii. o.	19	20	i. e.	7	3	20 i. t.	20	8	27
11	7	32 iii. e.	19	29	iii. o.	6	31	iii. O.	22	48	i. O.	5	33	i. T.	12	2	
9	53	iii. E.	21	52	iii. O.	15	31	i. e.	24	5	50 ii. e.	15	42	iii. e.	23	55	
10	34	iii. o.	26	11	43 i. e.	18	52	i. O.	8	18	ii. E.	17	51	iii. E.	21	2	23
11	29	i. t.	14	54	i. O.	10	0	38 ii. e.	8	25	ii. o.	21	21	iii. o.	7	10	
13	5	iii. O.	19	24	ii. e.	5	28	ii. O.	10	55	ii. O.	23	8	i. e.	9	22	
13	44	i. T.	23	56	ii. O.	14	0	i. t.	17	56	i. t.	23	17	iii. O.	23	44	
12	7	55 i. e.	27	10	1 i. t.	16	14	i. T.	20	9	i. T.	8	2	41 i. O.	22	1	51
10	54	i. O.	12	15	i. T.	11	10	0 i. e.	25	13	48 i. e.	11	2	ii. e.	2	56	
14	10	ii. e.	28	6	12 i. e.	13	22	i. O.	17	17	i. O.	13	29	ii. E.	5	33	
18	19	ii. O.	9	24	i. O.	21	8	ii. t.	26	2	35 ii. t.	13	46	ii. o.	6	30	
13	6	0 i. t.	15	36	ii. t.	23	39	ii. T.	5	5	ii. T.	16	15	ii. O.	7	20	
8	15	i. T.	18	9	ii. T.	12	8	29 i. t.	12	25	i. t.	21	49	i. t.	16	13	
14	2	24 i. e.	29	4	31 i. t.	10	43	i. T.	14	38	i. T.	9	0	2 i. T.	18	39	
5	25	i. O.	6	45	i. T.	18	35	iii. t.	27	3	11 iii. t.	17	36	i. e.	19	0	
10	1	ii. t.	9	50	iii. t.	20	47	iii. T.	5	14	iii. T.	21	10	i. O.	21	27	
12	35	ii. T.	12	11	iii. T.	13	4	28 i. e.	8	17	i. e.	10	7	58 ii. t.	23	1	38
15	0	30 i. t.	30	0	40 i. e.	7	52	i. O.	11	47	i. O.	10	26	ii. T.	3	51	
0	55	iii. t.	3	54	i. O.	13	56	ii. e.	19	8	ii. e.	16	18	i. t.	21	24	
2	45	i. T.	8	43	ii. e.	18	50	ii. O.	21	36	ii. E.	18	31	i. T.	24	0	59
3	25	iii. T.	13	19	ii. O.	14	2	59 i. t.	21	46	ii. o.	11	11	34 iii. t.	13	13	
20	52	i. e.	23	1	i. t.	5	13	i. T.	28	0	15 ii. O.	12	5	i. e.	15	41	
23	55	i. O.	31	1	15 i. T.	22	57	i. e.	6	55	i. t.	13	28	iii. T.	20	7	
16	3	29 ii. e.	19	9	i. e.	15	2	21 i. O.	9	8	i. T.	15	39	i. O.	22	19	
7	44	ii. O.	22	24	i. O.	10	30	ii. t.	29	2	45 i. e.	12	0	20 ii. e.	25	15	53
19	0	i. t.				13	1	ii. T.	6	16	i. O.	2	47	ii. E.	19	27	
21	15	i. T.				21	29	i. t.	15	56	ii. t.	3	5	ii. o.	19	42	
17	15	21 i. e.	June.			23	42	i. T.	18	26	ii. T.	5	33	ii. O.	21	27	
18	24	i. O.	1	4	59 ii. t.	16	3	38 iii. e.	30	1	24 i. t.	10	47	i. t.	26	5	30
23	24	ii. t.	7	32	ii. T.	5	53	iii. E.	3	37	i. T.	12	59	i. T.	7	57	
18	1	58 ii. T.	17	31	i. t.	8	38	iii. o.	11	41	iii. e.	13	6	33 i. e.	8	17	
11	33	iii. e.	19	35	iii. e.	10	48	iii. O.	13	52	iii. E.	10	8	i. O.	10	43	
13	30	i. t.	19	45	i. T.	17	25	i. e.	17	10	iii. o.	21	17	ii. t.	14	35	
13	53	iii. E.	21	52	iii. E.	20	51	i. O.	19	11	iii. O.	23	45	ii. T.	16	47	
15	2	iii. o.	23	54	iii. o.	17	3	14 ii. e.	21	14	i. e.	14	5	15 i. t.	27	10	21
15	45	i. T.	2	2	13 iii. O.	8	12	ii. O.	July.			7	28	i. T.	13	55	
17	29	iii. O.	13	37	i. e.	15	59	i. t.	1	0	45 i. O.	19	43	iii. e.	28	2	31
19	9	49 i. e.	16	54	i. O.	18	12	i. T.				21	51	iii. E.	4	5	

July (con.).			Aug. (con.).			Aug. (con.).			Sept. (con.).			Sept. (con.).			Oct. (con.).		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
28	9	3 i. t.	9	15	45 ii. O.	23	15	50 ii. e.	7	1	20 ii. O.	22	22	2 ii. t.	7	2	58 i. t.
	11	15 i. T.		18	19 i. t.		20	37 ii. O.		1	34 i. t.		23	30 i. t.		4	57 ii. T.
29	3	46 iii. e.		20	31 i. T.		21	58 i. t.		3	45 i. T.	23	0	25 ii. T.		5	9 i. T.
	4	50 i. e.	10	14	10 i. e.	24	0	10 i. T.		21	47 i. e.		1	41 i. T.		23	55 i. e.
	5	51 iii. E.		17	40 i. O.		17	58 i. e.	8	0	55 i. O.		20	5 i. e.	8	2	21 i. O.
	8	24 i. O.		11	7 37 ii. t.		21	20 i. O.		17	22 ii. t.		22	53 i. O.		20	2 iii. e.
	9	34 iii. o.		10	2 ii. T.	25	12	34 ii. t.		19	45 ii. T.	24	11	58 iii. e.		20	37 ii. e.
	11	16 iii. O.		12	47 i. t.		14	58 ii. T.		20	0 i. t.		13	51 iii. E.		21	23 i. t.
	18	48 ii. e.		14	59 i. T.		16	26 i. t.		22	11 i. T.		14	44 iii. o.		22	40 iii. C.
	21	14 ii. E.		12	8 38 i. e.		18	37 i. T.	9	16	16 i. e.		15	27 ii. e.		23	31 ii. O.
	21	33 ii. o.		11	49 iii. e.	26	12	27 i. e.		19	22 i. O.		16	3 iii. O.		23	35 i. T.
30	0	0 ii. O.		12	7 i. O.		15	47 i. O.	10	3	54 iii. e.		17	56 i. t.	9	18	23 i. e.
	3	31 i. t.		13	51 iii. E.		19	51 iii. e.		5	50 iii. E.		19	3 ii. O.		20	47 i. O.
	5	43 i. T.		17	21 iii. o.		21	50 iii. E.		7	55 iii. o.		20	8 i. T.	10	15	42 ii. t.
	23	18 i. e.		18	54 iii. O.	27	0	48 iii. o.		9	15 iii. O.	25	14	33 i. e.		15	49 i. t.
31	2	52 i. O.		23	58 ii. e.		2	13 iii. O.		10	17 ii. e.		17	19 i. O.		18	0 i. T.
	15	49 ii. t.	13	2	24 ii. E.		5	8 ii. e.		14	26 i. t.		26	11 10 ii. t.		18	5 ii. T.
	18	15 ii. T.		2	34 ii. o.		9	49 ii. O.		14	30 ii. O.		12	22 i. t.	11	12	52 i. e.
	22	0 i. t.		4	59 ii. O.		10	53 i. t.		16	37 i. T.		13	33 ii. T.		15	13 i. O.
				7	14 i. t.		13	4 i. T.	11	10	44 i. e.		14	34 i. T.	12	9	54 ii. e.
				9	26 i. T.	28	6	55 i. e.		13	48 i. O.	27	9	2 i. e.		10	15 i. t.
				14	3 7 i. e.		10	14 i. O.	12	6	32 ii. t.		11	45 i. O.		11	0 iii. t.
				6	35 i. O.	29	1	47 ii. t.		8	53 i. t.	28	4	29 iii. t.		12	24 iii. t.
				20	52 ii. t.		4	11 ii. T.		8	55 ii. T.		4	44 ii. e.		12	26 i. T.
				23	18 ii. T.		5	20 i. t.		11	4 i. T.		5	49 iii. T.		12	37 ii. O.
				15	1 42 i. t.		7	31 i. T.	13	5	13 i. e.		6	48 i. t.	13	7	21 i. e.
				3	53 i. T.	30	1	24 i. e.		8	15 i. O.		8	10 ii. O.		9	39 i. O.
				21	35 i. e.		4	41 i. O.		21	45 iii. t.		8	59 i. T.	14	4	41 i. t.
				16	1 3 i. O.		14	44 iii. t.		23	5 iii. T.	29	3	31 i. e.		4	49 ii. t.
				7	22 iii. t.		16	8 iii. T.		23	35 ii. e.		6	11 i. O.		6	52 i. T.
				8	53 iii. T.		18	25 ii. e.	14	3	19 i. t.	30	0	18 ii. t.		7	12 ii. T.
				13	15 ii. e.		23	0 ii. O.		3	39 ii. O.		1	14 i. t.	15	1	50 i. e.
				16	28 i. t.		23	46 i. e.		5	31 i. T.		2	41 ii. T.		4	5 i. O.
				18	40 i. T.	31	1	58 i. T.		23	41 i. e.		3	25 i. T.		23	6 i. t.
				3	12 15 i. e.		19	52 i. e.	15	2	41 i. O.		22	0 i. e.		23	12 ii. e.
				15	48 i. O.		23	8 i. O.		19	43 ii. t.				16	0	4 iii. e.
				4	5 5 ii. t.		22	21 i. T.		21	46 i. t.					1	18 i. T.
				7	31 ii. T.		17	16 4 i. e.		22	6 ii. T.					1	44 ii. O.
				10	56 i. t.		19	30 i. O.		23	57 i. T.					1	57 iii. O.
				13	8 i. T.		18	10 6 ii. t.	16	18	10 i. e.					20	18 i. e.
				5	6 44 i. e.		12	31 ii. T.		21	8 i. O.					22	31 i. O.
				7	47 iii. e.		14	37 i. t.	17	7	56 iii. e.					17	32 i. t.
				9	51 iii. E.		16	48 i. T.		9	51 iii. E.					17	57 ii. t.
				10	16 i. O.		19	10 32 i. e.		11	22 iii. o.					19	44 i. T.
				13	30 iii. o.		13	57 i. O.		12	41 iii. O.					20	20 ii. T.
				15	8 iii. O.		15	50 iii. e.		12	52 ii. e.					18	14 46 i. o.
				21	23 ii. e.		17	50 iii. E.		16	12 i. t.					16	57 i. O.
				23	49 ii. E.		21	7 iii. o.		16	47 ii. O.					19	11 58 i. t.
				6	0 5 ii. o.		22	36 iii. O.		18	23 i. T.					12	26 ii. o.
				2	31 ii. O.	20	2	33 ii. e.		18	12 39 i. e.					14	9 i. T.
				5	23 i. t.		4	59 ii. E.		15	34 i. O.					14	13 iii. t.
				7	35 i. T.		5	1 ii. o.		19	8 52 ii. t.					14	54 ii. E.
				1	12 i. e.		7	25 ii. O.		10	38 i. t.					15	42 iii. T.
				4	44 i. O.		9	4 i. t.		11	15 ii. T.					20	9 12 i. o.
				18	21 ii. t.		11	16 i. T.		12	49 i. T.					11	25 i. E.
				20	47 ii. T.		21	5 1 i. e.		20	7 7 i. e.					6	24 i. t.
				23	51 i. t.		8	25 i. O.		10	0 i. O.					7	4 ii. t.
				8	2 3 i. T.		23	21 ii. t.		21	1 9 iii. t.					8	35 i. T.
				19	41 i. e.		22	1 45 ii. T.		2	9 ii. e.					9	28 ii. T.
				23	12 i. O.		3	31 i. t.		2	28 iii. T.					22	3 38 i. o.
				9	3 33 iii. t.		5	43 i. T.		5	4 i. t.					5	54 i. E.
				5	8 iii. T.		23	29 i. e.		5	55 ii. O.					10	50 i. t.
				10	40 ii. e.	23	2	52 i. O.		7	15 i. T.					6	5 26 i. e.
				13	7 ii. E.		11	6 iii. t.	22	1	36 i. e.					7	55 i. O.
				13	20 ii. o.		12	33 iii. T.		4	27 i. O.					7	2 34 ii. t.

Oct. (con.).			Nov. (con.).			Nov. (con.).			Nov. (con.).			Dec. (con.).			Dec. (co		
d	h	m	d	h	m	d	h	m	d	h	m	d	h	m	d	h	m
23	4	12 ii. E.	4	11	35 ii. t.	16	21	7 i. T.	28	6	52 i. o.	9	20	57 i. T.	21	6	50
	5	53 iii. E.		12	3 i. T.		21	23 ii. o.		10	0 i. E.		23	21 ii. t.		10	16
	22	4 i. o.		14	0 ii. T.	17	1	16 ii. E.	29	4	1 i. t.	10	1	50 ii. T.	22	3	58
24	0	22 i. E.	5	7	6 i. o.		3	15 iii. t.		6	14 i. T.		16	3 i. o.		6	12
	19	16 i. t.		9	44 i. E.		5	5 iii. T.		7	44 ii. t.		19	22 i. E.		9	19
	20	12 ii. t.	6	4	17 i. t.		16	11 i. o.		10	12 ii. T.	11	13	12 i. t.		11	50
	21	27 i. T.		6	0 ii. o.		19	6 i. E.	30	1	19 i. o.		15	25 i. T.		11	53
	22	36 ii. T.		6	29 i. T.	18	13	21 i. t.		4	29 i. E.		17	38 ii. o.		14	18
25	16	30 i. o.		9	23 ii. E.		15	33 i. T.		22	28 i. t.		22	23 ii. E.		21	7
	18	51 i. E.		10	13 iii. o.		16	11 ii. t.				12	3	27 iii. o.		23	15
26	13	42 i. t.		12	11 iii. e.		18	38 ii. T.					5	32 iii. O.	23	1	18
	14	39 ii. o.		13	56 iii. E.	19	10	37 i. o.					8	24 iii. e.		4	45
	15	53 i. T.	7	1	32 i. o.		13	35 i. E.					10	2 iii. E.		22	26
	17	26 iii. t.		4	13 i. E.	20	7	47 i. t.		1	0 41 i. T.		10	30 i. o.	24	0	40
	17	29 ii. E.		22	43 i. t.		10	0 i. T.		2	2 ii. o.		13	51 i. E.		4	21
	18	59 iii. T.	8	0	44 ii. t.		10	32 ii. o.		6	28 ii. E.	13	7	39 i. t.		6	51
27	10	56 i. o.		0	55 i. T.		14	34 ii. E.		10	9 iii. t.		9	52 i. T.		19	46
	13	20 i. E.		3	9 ii. T.		16	53 iii. o.		12	8 iii. T.		12	35 ii. t.		23	14
28	8	7 i. t.		19	59 i. o.		18	45 iii. O.		19	46 i. o.		15	5 ii. T.	25	16	55
	9	19 ii. t.		22	42 i. E.		20	17 iii. e.		22	58 i. E.		14	4 58 i. o.		19	8
	10	19 i. T.	9	17	9 i. t.		21	59 iii. E.		2	16 55 i. t.		8	20 i. E.		22	34
	11	43 i. T.		19	7 ii. o.	21	5	4 i. o.		19	8 i. T.	15	2	7 i. t.	26	1	5
29	5	22 i. o.		19	21 i. T.		8	4 i. E.		20	56 ii. t.		4	20 i. T.		1	12
	7	49 i. E.		22	40 ii. E.	22	2	14 i. t.		23	24 ii. T.		6	51 ii. o.		3	36
30	2	33 i. t.		23	56 iii. t.		4	26 i. T.		3	14 13 i. o.		11	41 ii. E.		10	58
	3	46 ii. o.	10	1	40 iii. T.		5	22 ii. t.		17	27 i. E.		17	22 iii. t.		13	8
	4	45 i. T.		14	25 i. o.		7	49 ii. T.		4	11 23 i. t.		19	28 iii. T.		14	14
	6	47 ii. E.		17	11 i. E.		23	31 i. o.		13	35 i. T.		23	26 i. o.		16	30
	6	57 iii. o.	11	11	35 i. t.	23	2	33 i. E.		15	13 ii. o.	16	2	49 i. E.		17	43
	9	54 iii. E.		13	47 i. T.		20	41 i. t.		19	46 ii. E.		20	35 i. t.		18	6
	23	48 i. o.		13	52 ii. t.		22	53 i. T.		23	51 iii. o.		22	48 i. T.	27	11	23
31	2	18 i. E.		16	18 ii. T.		23	41 ii. o.		5	1 51 iii. O.	17	1	49 ii. t.		13	36
	20	59 i. t.	12	8	51 i. o.		24	3 52 ii. E.		4	21 iii. e.		4	19 ii. T.		17	37
	22	27 ii. t.		11	40 i. E.		6	39 iii. t.		6	1 iii. E.		17	54 i. o.		20	8
	23	11 i. T.	13	6	2 i. t.		8	34 iii. T.		8	40 i. o.		21	18 i. E.	28	8	43
				8	14 i. T.		17	58 i. o.		11	56 i. E.	18	15	2 i. t.		12	12
				8	15 ii. o.		21	2 i. E.		6	50 i. t.		17	16 i. T.	29	5	51
				11	58 ii. E.	25	15	7 i. t.		8	3 i. T.		20	4 ii. o.		8	4
				13	31 iii. o.		17	20 i. T.		10	8 ii. t.		22	35 ii. O.		11	50
				15	18 iii. O.		18	32 ii. t.		12	37 ii. T.		22	35 ii. e.		14	21
1	0	52 ii. T.		16	14 iii. e.		21	0 ii. T.		7	3 8 i. o.	19	1	0 ii. E.		14	30
	18	14 i. o.		17	57 iii. E.	26	12	25 i. o.		6	25 i. E.		7	10 iii. o.		16	55
	20	47 i. E.		15	18 iii. O.		15	31 i. E.		8	0 17 i. t.		9	17 iii. O.	30	0	58
2	15	25 i. t.	14	3	18 i. o.		17	9 34 i. t.		2	30 i. T.		12	22 i. o.		3	8
	16	53 ii. o.		6	9 i. E.		11	47 i. T.		4	25 ii. o.		12	27 iii. e.		3	11
	17	37 i. T.		15	0 28 i. t.		12	52 ii. o.		9	5 ii. E.		14	4 iii. E.		6	41
	20	5 ii. E.		2	40 i. T.		17	10 ii. E.		13	43 iii. t.		15	47 i. E.	31	0	19
	20	39 iii. t.		3	2 ii. t.		20	19 iii. o.		15	45 iii. T.		20	9 30 i. t.		2	33
	22	18 iii. T.		5	28 ii. T.		22	16 iii. O.		21	35 i. o.		11	44 i. T.		6	54
3	12	40 i. o.		21	44 i. o.	28	0	19 iii. e.		9	0 53 i. E.		15	5 ii. t.		9	25
	15	15 i. E.	16	0	38 i. E.		2	0 iii. E.		18	44 i. t.		17	35 ii. T.		21	39
4	9	51 i. t.		18	54 i. t.												

The Satellites of Jupiter will be invisible from March 1 until April 23, Jupiter being too near the Sun.

Satellites I. & II. *reappear* at eclipse on the *f* side in January, February, November, a December, and *disappear* on the *p* side from April till October. Satellite II. *reappears* on the *p* side in July and August.

Satellite III. *disappears* and *reappears* on the *f* side in January, February, November, a December, and on the *p* side from April until October.

Satellite IV. *disappears* and *reappears* on the *f* side in January and February. There will be no eclipse of the satellite in the other months.

CONFIGURATIONS OF JUPITER'S SATELLITES FOR AN INVERTING TELESCOPE.

Day.	Jan. 7 ^a	Feb. 6 ^h	April. 16 ^h	May. 15 ^h	June. 14 ^h	July. 13 ^h	Aug. 13 ^h	Sept. 12 ^h	Oct. 12 ^h	Nov. 10 ^h	Dec. 9 ^h	Day.
1	32014	21034		32104	23014	0134	42103	43021	42301	21043	10324	1
2	31024	20134		21302	2034	10234	34016	42310	41023	20413	30124	2
3	1024	0234		43012	10234	20134	3042	40123	40213	43102	32104	3
4	20143	10324		42103	01243	2304	2304	40123	4203	24302	32014	4
5	12043	32014		40113	21240	31024	23014	21043	41302	43201	0432	5
6	40312	31204		41023	4301	30214	10234	20314	43012	41302	41023	6
7	43102	31024		42031	43102	21304	02134	31024	34210	40123	42013	7
8	43201	21023		43210	43201	20413	21034	30214	23401	41203	4103	8
9	43102	42013		34012	42103	41023	5014	3104	10324	42013	43012	9
10	2402	4023		3402	24023	42013	31042	0134	02134	31402	43210	10
11	4203	41032		01034	40123	2420	23240	0234	21034	30124	43201	11
12	42103	43201		21034	42103	43102	4201	21043	30124	3204	4102	12
13	40132	43210		10234	3401	43012	41023	20413	30124	1304	24023	13
14	13024	43012		20314	31042	42310	40123	43102	31204	01234	20143	14
15	32014	4102		23104	32014	42013	42103	43021	23014	12034	1034	15
16	3104	34013		30124	21034	41023	43201	43210	10432	20134	30124	16
17	31024	1043		3024	01234	20413	43102	4201	40213	13024	31204	17
18	2034	21034		2104	0234	21034	34201	42013	42103	30124	32014	18
19	21034	32014		24013	21034	23024	2340	24203	24203	32410	1024	19
20	01234	32104		41023	32014	31024	10234	42013	43012	24320	01234	20
21	13024	31024		42013	31042	32104	01234	34102	43201	40112	2043	21
22	32041	13024		42103	23401	20314	21034	30412	43201	41203	10423	22
23	34120	20134	24301	43021	4210	10234	23014	32104	41032	42013	43012	23
24	43012	12043	3204	43102	40123	21043	31024	2014	40123	41302	43120	24
25	42103	40132	31024	42013	4023	21043	23014	10234	21043	43012	43201	25
26	24203	4320	23014	42013	24203	34012	2304	21034	20134	34210	41302	26
27	40123	43210	21034	14023	43201	4302	10243	20134	3024	23240	40123	27
28	24102	43012	02134	02143	43102	43210	40123	31024	31204	01324	4203	28
29	43201	41302	10234	21304	34021	42031	42103	30142	32014	21034	42103	29
30	31420		21014	30214	21304	41023	42031	32104	10324	20134	43012	30
31	30412			31024		40213	43102		01234		23104	31

The circle (O) represents Jupiter; 2 signifies that the satellite is on the disk;
● signifies that the satellite is behind the disk or in the shadow.

EPEMERIS OF JUPITER'S SATELLITE V.

Greenwich Mean Time of every twentieth Eastern Elongation. The times of other Elongations can be found by adding multiples of 11^h 96 to these.

	d	h		d	h		d	h		d	h
June	10	4 ^h 69	Aug.	9	23 ^h 46	Oct.	8	18 ^h 02	Dec.	7	12 ^h 63
	20	3 ^h 85		19	22 ^h 57		18	17 ^h 10		17	11 ^h 77
	30	3 ^h 01		29	21 ^h 68		28	16 ^h 19		27	10 ^h 92
July	10	2 ^h 16	Sept.	8	20 ^h 78	Nov.	7	15 ^h 28			
	20	1 ^h 30		18	19 ^h 86		17	14 ^h 38			
	30	0 ^h 43		28	18 ^h 94		27	13 ^h 50			

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
0	"	0	"	0	"	0	"
0	0 ^o	56	1 25 ^h 4	68	2 21 ^h 9	78	4 25 ^h 0
10	10 ^h 2	58	1 32 ^h 1	69	2 29 ^h 3	79	4 48 ^h 5
20	21 ^h 0	60	1 39 ^h 7	70	2 37 ^h 3	80	5 16 ^h 2
30	33 ^h 3	61	1 43 ^h 8	71	2 46 ^h 1	81	5 49 ^h 3
35	40 ^h 4	62	1 48 ^h 2	72	2 55 ^h 8	82	6 29 ^h 6
40	48 ^h 4	63	1 52 ^h 8	73	3 6 ^h 6	83	7 19 ^h 7
45	57 ^h 7	64	1 57 ^h 8	74	3 18 ^h 6	84	8 23 ^h 3
50	1 8 ^h 7	65	2 3 ^h 2	75	3 32 ^h 1	85	9 46 ^h 5
52	1 13 ^h 8	66	2 8 ^h 9	76	3 47 ^h 4		
54	1 19 ^h 3	67	2 15 ^h 2	77	4 4 ^h 9		

SATELLITES OF SATURN.

East Elongations.

MIMAS.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
1 6.9	1 10.8	1 13.1	1 16.8	1 19.1	1 21.3	1 2.5	1 4.9	1 8.9
2 5.5	2 9.4	2 11.8	2 15.4	2 17.7	2 20.0	2 1.1	2 3.5	2 7.5
3 4.1	3 8.0	3 10.4	3 14.1	3 16.3	3 18.6	2 23.7	3 2.1	3 6.1
4 2.8	4 6.6	4 9.0	4 12.7	4 14.9	4 17.2	3 22.3	4 0.8	4 4.8
5 1.4	5 5.2	5 7.6	5 11.3	5 13.5	5 15.8	4 20.9	4 23.4	5 3.4
6 0.0	6 3.9	6 6.2	6 9.9	6 12.1	6 14.4	5 19.5	5 22.0	6 2.0
6 22.6	7 2.5	7 4.8	7 8.5	7 10.7	7 13.0	6 18.1	6 20.6	7 0.6
7 21.3	8 1.1	8 3.4	8 7.1	8 9.4	8 11.6	7 16.8	7 19.3	7 23.2
8 19.9	8 23.7	9 2.1	9 5.7	9 8.0	9 10.2	8 15.4	8 17.9	8 21.9
9 18.5	9 22.3	10 0.7	10 4.3	10 6.6	10 8.9	9 14.0	9 16.5	9 20.5
10 17.1	10 21.0	10 23.3	11 3.0	11 5.2	11 7.5	10 12.6	10 15.1	10 19.1
11 15.7	11 19.6	11 21.9	12 1.6	12 3.8	12 6.1	11 11.2	11 13.8	11 17.7
12 14.4	12 18.2	12 20.5	13 0.2	13 2.4	13 4.7	12 9.8	12 12.4	12 16.3
13 13.0	13 16.8	13 19.1	13 22.8	14 1.0	14 3.3	13 8.5	13 11.0	13 15.0
14 11.6	14 15.4	14 17.8	14 21.4	14 23.7	15 1.9	14 7.1	14 9.6	14 13.6
15 10.2	15 14.0	15 16.4	15 20.0	15 22.3	16 0.5	15 5.7	15 8.3	15 12.2
16 8.8	16 12.7	16 15.0	16 18.6	16 20.9	16 23.2	16 4.3	16 6.9	16 10.8
17 7.5	17 11.3	17 13.6	17 17.3	17 19.5	17 21.8	17 2.9	17 5.5	17 9.5
18 6.1	18 9.9	18 12.2	18 15.9	18 18.1	18 20.4	18 1.6	18 4.1	18 8.1
19 4.7	19 8.5	19 10.8	19 14.5	19 16.7	19 19.0	19 0.2	19 2.7	19 6.7
20 3.3	20 7.1	20 9.5	20 13.1	20 15.3	20 17.6	19 22.8	20 1.4	20 5.4
21 1.9	21 5.7	21 8.1	21 11.7	21 13.9	21 16.3	20 21.4	21 0.0	
22 0.6	22 4.4	22 6.7	22 10.3	22 12.5	22 14.9	21 20.0	21 22.6	
22 23.2	23 3.0	23 5.3	23 8.9	23 11.2	23 13.5	22 18.7	22 21.2	
23 21.8	24 1.6	24 3.9	24 7.5	24 9.8	24 12.1	23 17.3	23 19.8	
24 20.4	25 0.2	25 2.5	25 6.2	25 8.4	25 10.7	24 15.9	24 18.4	
25 19.0	25 22.8	26 1.1	26 4.8	26 7.0	26 9.4	25 14.5	25 17.0	
26 17.7	26 21.5	26 23.7	27 3.4	27 5.6	27 8.0	26 13.2	26 15.7	
27 16.3	27 20.1	27 22.4	28 2.0	28 4.3	28 6.6	27 11.8	27 14.3	
28 14.9	28 18.7	28 21.0	29 0.6	29 2.9	29 5.2	28 10.4	28 12.9	
29 13.5	29 17.3	29 19.6	29 23.2	30 1.5	30 3.8	29 9.0	29 11.6	
30 12.1	30 15.9	30 18.2	30 21.8	31 0.1	31 2.7	30 7.7	30 10.2	
	31 14.5		31 20.5	31 22.7		31 6.3		

ENCELADUS.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
2 8.8	1 3.5	1 15.9	1 19.2	2 7.3	1 10.6	1 14.0	2 2.4	2 6.1
3 17.7	2 12.4	3 0.8	3 4.1	3 16.2	2 19.5	2 22.9	3 11.3	3 15.0
5 2.6	3 21.3	4 9.6	4 13.0	5 1.1	4 4.4	4 7.8	4 20.2	4 23.9
6 11.5	5 6.2	5 18.5	5 21.9	6 10.0	5 13.3	5 16.6	6 5.1	6 8.8
7 20.4	6 15.1	7 3.4	7 6.8	7 18.9	6 22.2	7 1.5	7 14.0	7 17.7
9 5.3	8 0.0	8 12.3	8 15.6	9 3.7	8 7.0	8 10.4	8 22.9	9 2.6
10 14.2	9 8.9	9 21.2	10 0.5	10 12.6	9 15.9	9 19.3	10 7.8	10 11.5
11 23.1	10 17.8	11 6.0	11 9.4	11 21.5	11 0.8	11 4.2	11 16.7	11 20.4
13 8.0	12 2.7	12 14.9	12 18.2	13 6.4	12 9.7	12 13.1	13 1.6	13 5.3
14 16.8	13 11.6	13 23.8	14 3.1	14 15.2	13 18.6	13 22.0	14 10.5	14 14.2
16 1.7	14 20.4	15 8.7	15 12.0	16 0.1	15 3.5	15 6.9	15 19.4	15 23.1
17 10.6	16 5.3	16 17.6	16 20.8	17 9.0	16 12.3	16 15.7	17 4.2	17 8.0
18 19.5	17 14.2	18 2.4	18 5.7	18 17.9	17 21.2	18 0.6	18 13.1	18 16.9
20 4.4	18 23.1	19 11.3	19 14.6	20 2.7	19 6.1	19 9.5	19 22.0	20 1.8
21 13.3	20 8.0	20 20.2	20 23.5	21 11.6	20 15.0	20 18.4	21 6.9	
22 22.2	21 16.8	22 5.1	22 8.3	22 20.5	21 23.8	22 3.3	22 15.8	
24 7.1	23 1.7	23 14.0	23 17.2	24 5.4	23 8.7	23 12.2	24 0.7	
25 16.0	24 10.6	24 22.8	25 2.1	25 14.2	24 17.6	24 21.1	25 9.6	
27 0.9	25 19.5	26 7.7	26 11.0	26 23.1	26 2.5	26 5.9	26 18.5	
28 9.8	27 4.4	27 16.6	27 19.9	28 8.0	27 11.4	27 14.8	28 3.4	
29 18.7	28 13.2	29 1.5	29 4.8	29 16.9	28 20.2	28 23.7	29 12.3	
	29 22.1	30 10.4	30 13.6	31 1.8	30 5.1	30 8.6	30 21.2	
	31 7.0		31 22.5			31 17.5		

East Elongations.

TETHYS.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
1 2'2	1 7'3	2 9'6	2 14'3	1 19'0	2 20'9	1 4'3	2 6'6	2 11'8
2 23'5	3 4'7	4 6'9	4 11'6	3 16'3	4 18'2	3 1'6	4 3'9	4 9'1
4 20'8	5 2'0	6 4'2	6 8'9	5 13'5	6 15'5	4 22'9	6 1'2	6 6'4
6 18'2	6 23'3	8 1'5	8 6'2	7 10'8	8 12'7	6 20'2	7 22'6	8 3'7
8 15'5	8 20'6	9 22'8	10 3'5	9 8'1	10 10'0	8 17'5	9 19'9	10 1'0
10 12'8	10 17'9	11 20'1	12 0'8	11 5'4	12 7'3	10 14'8	11 17'2	11 22'4
12 10'1	12 15'2	13 17'4	13 22'1	13 2'7	14 4'6	12 12'1	13 14'5	13 19'7
14 7'5	14 12'5	15 14'7	15 19'4	15 0'0	16 1'9	14 9'5	15 11'8	15 17'0
16 4'8	16 9'8	17 12'0	17 16'7	16 21'2	17 23'2	16 6'8	17 9'2	17 14'3
18 2'1	18 7'1	19 9'3	19 14'0	18 18'5	19 20'5	18 4'1	19 6'5	
19 23'4	20 4'4	21 6'6	21 11'2	20 15'8	21 17'8	20 1'4	21 3'8	
21 20'8	22 1'7	23 3'9	23 8'5	22 13'1	23 15'1	21 22'7	23 1'1	
23 18'1	23 23'1	25 1'2	25 5'8	24 10'4	25 12'4	23 20'0	24 22'5	
25 15'4	25 20'4	26 22'5	27 3'1	26 7'7	27 9'7	25 17'3	26 19'8	
27 12'7	27 17'7	28 19'8	29 0'4	28 5'0	29 7'0	27 14'7	28 17'1	
29 10'0	29 15'0	30 17'0	30 21'7	30 2'3		29 12'0	30 14'4	
	31 12'3			31 23'6		31 9'3		

DIONE.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
2 13'4	2 16'3	1 18'8	1 21'2	3 17'0	2 19'1	2 21'4	2 0'0	2 2'9
5 7'1	5 10'0	4 12'5	4 14'8	6 10'6	5 12'7	5 15'1	4 17'7	4 20'6
8 0'8	8 3'7	7 6'2	7 8'5	9 4'3	8 6'4	8 8'7	7 11'4	7 14'3
10 18'5	10 21'3	9 23'9	10 2'1	11 21'9	11 0'0	11 2'4	10 5'1	10 8'1
13 12'2	13 15'0	12 17'5	12 19'8	14 15'6	13 17'7	13 20'1	12 22'8	13 1'8
16 6'0	16 8'7	15 11'2	15 13'4	17 9'2	16 11'4	16 13'8	15 16'5	15 19'5
18 23'7	19 2'4	18 4'9	18 7'1	20 2'8	19 5'0	19 7'5	18 10'3	18 13'3
21 17'4	21 20'1	20 22'5	21 0'7	22 20'5	21 22'7	22 1'2	21 4'0	
24 11'1	24 13'8	23 16'2	23 18'4	25 14'1	24 16'4	24 18'9	23 21'7	
27 4'8	27 7'5	26 9'8	26 12'0	28 7'8	27 10'1	27 12'6	26 15'4	
29 22'5	30 1'1	29 3'5	29 5'7	31 1'4	30 3'7	30 6'3	29 9'2	
			31 23'3					

RHEA.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
2 10'7	4 2'3	4 17'3	1 19'5	2 9'8	3 0'0	4 14'5	5 5'5	2 8'6
6 23'2	8 14'8	9 5'7	6 7'9	6 22'1	7 12'3	9 2'9	9 18'0	6 21'1
11 11'8	13 3'2	13 18'1	10 20'2	11 10'4	12 0'7	13 15'3	14 6'5	11 9'6
16 0'3	17 15'6	18 6'4	15 8'5	15 22'7	16 13'0	18 3'7	18 19'0	15 22'1
20 12'8	22 4'1	22 18'8	19 20'8	20 11'0	21 1'4	22 16'2	23 7'5	20 10'7
25 1'3	26 16'5	27 7'2	24 9'2	24 23'3	25 13'7	27 4'6	27 20'0	
29 13'8	31 4'9		28 21'5	29 11'7	30 2'1	31 17'1		

TITAN.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
1 21'8	3 21'5	4 19'6	6 15'9	7 10'8	8 5'7	10 1'6	10 23'5	12 22'9
17 21'9	19 20'8	20 17'9	22 13'4	23 8'1	24 3'5	26 0'3	26 23'2	

East Elongations.

HYPERION.

April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h
16 9 ¹	7 17 ⁸	19 5 ³	10 9 ¹	21 13 ²	11 14 ⁹	2 17 ⁵	14 2 ²	5 8 ²
	29 0 ⁵		31 11 ⁵			23 21 ⁴		

IAPETUS.

	d h	d h	d h	d h
E. Elongation	May 1 19 ⁴	July 19 19 ⁴	Oct. 5 23 ⁵	
Inf. Conjunction ...	May 21 16 ³	Aug. 8 6 ²	Oct. 25 19 ⁰	
W. Elongation	June 11 4 ³	Aug. 28 9 ⁴	Nov. 15 10 ⁶	
Sup. Conjunction ...	Apr. 12 9 ⁴	June 30 23 ⁰	Sept. 17 0 ⁵	Dec. 5 13 ²

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
Jan. 10	0 17 ¹	35 ⁵⁵	10 ⁹⁹	0 0 ¹ N.	0 17 ⁷ N.
30	7 13 ¹	35 ³⁵	10 ³⁹	17 5 ⁹ N.	17 4 ⁴ N.
Feb. 19	7 8 ⁴	35 ⁴⁸	9 ⁸⁸	16 10 ² N.	16 51 ⁰ N.
Mar. 10	7 3 ⁴	35 ⁹³	9 ⁴⁷	15 16 ⁷ N.	16 37 ⁶ N.
30	6 58 ⁶	36 ⁷⁰	9 ¹⁹	14 29 ⁷ N.	16 24 ⁰ N.
April 19	6 54 ⁷	37 ⁷⁴	9 ⁰⁵	13 52 ⁷ N.	16 10 ³ N.
May 9	6 51 ⁸	38 ⁹⁸	9 ⁰⁹	13 29 ⁴ N.	15 56 ⁵ N.
29	6 50 ⁸	40 ³³	9 ³²	13 22 ¹ N.	15 42 ⁶ N.
June 18	6 51 ⁵	41 ⁶²	9 ⁷³	13 31 ⁵ N.	15 28 ⁶ N.
July 8	6 53 ⁹	42 ⁶⁹	10 ²⁸	13 55 ⁹ N.	15 14 ⁴ N.
28	6 57 ³	43 ³³	10 ⁸⁷	14 31 ² N.	15 0 ² N.
Aug. 17	7 1 ²	43 ⁴³	11 ³⁷	15 10 ³ N.	14 45 ⁹ N.
Sept. 6	7 4 ⁵	42 ⁹⁶	11 ⁶⁷	15 45 ⁵ N.	14 31 ⁴ N.
26	7 6 ⁷	42 ⁰¹	11 ⁷⁰	16 10 ² N.	14 16 ⁹ N.
Oct. 16	7 7 ⁷	40 ⁷⁷	11 ⁴⁷	16 20 ¹ N.	14 2 ² N.
Nov. 5	7 7 ²	39 ⁴²	11 ⁰¹	16 13 ² N.	13 47 ⁵ N.
25	7 5 ¹	38 ¹⁵	10 ⁴¹	15 50 ¹ N.	13 32 ⁷ N.
Dec. 15	7 1 ⁵	37 ⁰⁷	9 ⁷²	15 12 ⁶ N.	13 17 ⁸ N.

SATELLITES OF URANUS.

East Elongations.

ARIEL.

April.	May.	June.	July.	August.	September.	October.
d h	d h	d h	d h	d h	d h	d h
13 9 ^s	1 1 ^o	2 19 ⁷	3 2 ^o	2 8 ³	1 14 ⁵	1 20 ⁴
15 22 ^o	3 13 ⁵	5 8 ²	5 14 ⁶	4 20 ⁹	4 3 ^o	4 8 ⁸
18 10 ⁵	6 2 ^o	7 20 ⁷	8 3 ¹	7 9 ⁴	6 15 ⁵	6 21 ³
20 23 ^o	8 14 ⁵	10 9 ²	10 15 ⁶	9 21 ⁹	9 4 ^o	9 9 ⁸
23 11 ⁵	11 3 ^o	12 21 ⁸	13 4 ²	12 10 ⁵	11 16 ⁵	11 22 ²
26 0 ^o	13 15 ⁶	15 10 ³	15 16 ⁷	14 23 ^o	14 5 ^o	
28 12 ⁵	16 4 ¹	17 22 ⁸	18 5 ²	17 11 ⁵	16 17 ⁵	
	18 16 ⁶	20 11 ⁴	20 17 ⁸	20 0 ^o	19 6 ^o	
	21 5 ¹	23 0 ^o	23 6 ³	22 12 ⁵	21 18 ⁵	
	23 17 ⁶	25 12 ⁵	25 18 ⁸	25 1 ^o	24 7 ^o	
	26 6 ¹	28 1 ^o	28 7 ³	27 13 ⁵	26 19 ⁴	
	28 18 ⁷	30 13 ⁵	30 19 ⁸	30 2 ^o	29 7 ⁹	
	31 7 ²					

UMBRIEL.

April.	May.	June.	July.	August.	September.	October.
d h	d h	d h	d h	d h	d h	d h
8 5 ⁹	3 2 ⁸	1 3 ⁴	4 7 ⁸	2 9 ^o	4 13 ¹	3 13 ¹
12 9 ⁴	7 6 ³	5 7 ^o	8 11 ⁶	6 12 ⁵	8 16 ⁶	7 16 ⁵
16 12 ⁸	11 9 ⁸	9 10 ⁵	12 15 ¹	10 16 ¹	12 20 ^o	11 19 ⁸
20 16 ³	15 13 ³	13 14 ¹	16 18 ⁷	14 19 ⁶	16 23 ⁵	15 23 ²
24 19 ⁸	19 16 ⁸	17 17 ⁶	20 22 ²	18 23 ¹	21 2 ⁸	20 2 ⁶
28 23 ³	23 20 ³	21 21 ³	25 1 ⁹	23 2 ⁶	25 6 ³	24 5 ⁹
	27 23 ⁹	26 0 ⁸	29 5 ⁴	27 6 ¹	29 9 ⁷	
		30 4 ⁴		31 9 ⁶		

TITANIA.

March.	May.	June.	July.	August.	September.	October.
d h	d h	d h	d h	d h	d h	d h
29 21 ¹	3 17 ²	7 14 ³	3 18 ⁶	7 16 ¹	2 19 ⁵	7 14 ⁶
	12 10 ⁴	16 7 ⁷	12 12 ^o	16 9 ³	11 12 ⁴	16 7 ¹
April.	21 3 ⁶	25 1 ¹	21 5 ⁴	25 2 ⁵	20 5 ³	24 23 ⁶
7 14 ^o	29 20 ⁹		29 22 ⁸		28 22 ^o	
16 7 ^o						
25 0 ^o						

OBERON.

April.	May.	June.	July.	August.	September.	October.
d h	d h	d h	d h	d h	d h	d h
28 12 ⁷	12 0 ⁴	8 0 ⁴	5 0 ⁹	1 1 ³	10 12 ^o	7 9 ¹
	25 12 ³	21 12 ⁶	18 13 ²	14 13 ²	23 22 ⁷	
				28 0 ⁸		

Apparent Apsides.

Apparent Distances.

	Position-Angle.	Ariel.	Umbriel.	Titania.	Oberon.
April 23	211 ¹	14 ¹	19 ⁷	32 ³	43 ²
June 22	216 ^o	14 ⁵	20 ²	33 ¹	44 ²
August 21	221 ⁴	14 ¹	19 ⁶	32 ¹	43 ^o

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.	February.	March.	April.	October.	November.	December.
d h	d h	d h	d h	d h	d h	d h
3 3 ^o	1 12 ⁶	1 22 ¹	6 4 ⁴	5 6 ³	3 15 ⁵	3 1 ^o
9 0 ¹	7 9 ⁷	7 19 ²		11 3 ³	9 12 ⁶	8 22 ¹
14 21 ²	13 6 ⁸	13 16 ²	September.	17 0 ³	15 9 ⁷	14 19 ²
20 18 ⁴	19 3 ⁹	19 13 ²		22 21 ⁴	21 6 ⁷	20 16 ³
26 15 ⁵	25 1 ^o	25 10 ³	23 12 ³	28 18 ⁴	27 3 ⁸	26 13 ⁵
		31 7 ⁴	29 9 ³			

Position-angle and distance of the apse:—Jan. 18, 265^o 4, 16^{''} 8;
 March 22, 264^o 4, 16^{''} 3; Oct. 24, 270^o 5, 16^{''} 5; Dec. 27, 268^o 8, 16^{''} 9.

**EPHEMERIS FOR PHYSICAL OBSERVATIONS
OF THE SUN.**

Greenwich Mean Noon.				Greenwich Mean Noon.			
1904.	P.	D.	L.	1904.	P.	D.	L.
Jan. 1	+ 0 22	- 0 8	203 49	July 4	- 0 14	+ 0 24	282 5
6	- 0 4	3 42	137 58	9	+ 1 2	3 55	215 54
11	2 29	4 14	72 7	14	3 19	4 25	149 44
16	4 53	4 45	6 17	19	5 31	4 53	83 34
21	7 12	5 13	300 27	24	7 41	5 18	17 25
26	9 27	5 38	234 37	29	9 47	5 42	311 17
31	11 36	6 1	166 47	Aug. 3	11 47	6 3	245 9
Feb. 5	13 38	6 21	102 57	8	13 42	6 22	179 2
10	15 33	6 38	37 7	13	15 31	6 38	112 57
15	17 20	6 52	331 17	18	17 13	6 51	46 52
20	18 58	7 3	265 27	23	18 47	7 2	340 48
25	20 28	7 10	199 36	28	20 15	7 9	274 44
Mar. 1	21 48	7 14	133 44	Sept. 2	21 34	7 14	208 40
6	22 59	7 15	67 51	7	22 44	7 15	142 38
11	24 0	7 12	1 57	12	23 45	7 13	76 37
16	24 51	7 6	296 3	17	24 38	7 8	10 38
21	25 31	6 57	230 9	22	25 20	7 0	304 38
26	26 1	6 45	164 14	27	25 53	6 49	238 38
31	26 21	6 30	98 16	Oct. 2	26 16	6 34	172 39
Apr. 5	26 29	6 12	32 16	7	26 27	6 17	106 41
10	26 27	5 51	326 17	12	26 29	5 57	40 44
15	26 14	5 27	260 17	17	26 19	5 34	334 47
20	25 49	5 2	194 15	22	25 57	5 9	268 50
25	25 14	4 34	128 12	27	25 24	4 41	202 54
30	24 27	4 4	62 8	Nov. 1	24 39	4 11	136 58
May 5	23 30	3 33	356 2	6	23 42	3 39	71 2
10	22 22	3 0	289 55	11	22 33	3 6	5 7
15	21 4	2 27	223 48	16	21 12	2 30	299 11
20	19 36	1 52	157 39	21	19 41	1 54	233 16
25	17 58	1 17	91 29	26	17 58	1 16	167 22
30	16 11	0 41	25 19	Dec. 1	16 6	+ 0 38	101 29
June 4	14 18	- 0 4	319 9	6	14 4	0 0	35 36
9	12 18	+ 0 32	252 59	11	11 56	- 0 38	329 43
14	10 11	1 8	186 48	16	9 41	1 16	263 50
19	8 2	1 43	120 37	21	7 19	1 54	197 58
24	5 48	2 18	54 26	26	4 55	2 31	132 7
29	- 3 31	+ 2 51	348 15	31	+ 2 30	- 3 6	66 16

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be $82^{\circ} 45'$, and the longitude of the ascending node to be $74^{\circ} 25'$. In computing L (the heliographic longitude of the centre of the Sun's disk), the Sun's period of rotation has been assumed to be 25.38 days, and the meridian which passed through the ascending node at the epoch 1854.0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1904°0.

No.	Star.	R.A.			Dec.	No.	Star.	R.A.			Dec.
		h	m	s	°			h	m	s	°
1.	V Sculptoris ...	0	3	46	-39 45'8	58.	X Camelopardi.	4	33	7	+74 56'5
2.	S Sculptoris ...	0	10	31	-32 34'8	59.	R Doradus	4	35	39	-62 16'0
3.	T Andromedæ ...	0	17	23	+26 27'8	60.	R Cæli	4	37	10	-38 25'3
4.	T Cassiopeiæ ...	0	18	2	+55 15'6	61.	R Pictoris	4	43	35	-49 25'2
5.	S Toucani	0	18	35	-62 12'5	62.	V Tauri	4	46	28	+17 22'6
6.	R Andromedæ ...	0	18	57	+38 2'8	63.	U Leporis	4	52	11	-21 22'2
7.	S Ceti	0	19	10	-9 51'6	64.	R Orionis	4	53	48	+7 59'2
8.	T Sculptoris ...	0	24	29	-38 26'3	65.	R Leporis	4	55	14	-14 57'1
9.	Y Cephei	0	31	35	+79 49'7	66.	T Leporis	5	0	45	-22 2'1
10.	U Cassiopeiæ ...	0	40	59	+47 43'9	67.	V Orionis	5	1	0	+3 58'3
11.	V Andromedæ ...	0	44	52	+35 7'8	68.	S Pictoris	5	8	24	-48 37'4
12.	X Sculptoris ...	0	44	53	-35 26'5	69.	R Aurigæ	5	9	33	+53 28'7
13.	R ² Andromedæ ...	0	46	10	+33 51'3	70.	T Columbae	5	15	46	-33 48'4
14.	W Cassiopeiæ ...	0	49	13	+58 2'6	71.	W Aurigæ	5	20	25	+36 49'1
15.	U Cephei	0	53	44	+81 21'5	72.	Y Aurigæ	5	21	49	+42 21'4
16.	U Sculptoris ...	1	7	1	-30 37'5	73.	S Orionis	5	24	16	-4 46'1
17.	U Andromedæ ...	1	10	1	+40 12'7	74.	S Camelopardi ...	5	30	39	+68 44'8
18.	S Piscium	1	12	33	+8 25'5	75.	U Aurigæ	5	35	54	+31 59'6
19.	S Cassiopeiæ ...	1	12	35	+72 6'4	76.	S Columbae	5	43	19	-31 43'6
20.	U Piscium	1	17	54	+12 21'9	77.	R Columbae	5	46	49	-29 13'1
21.	R Sculptoris ...	1	22	33	-33 2'4	78.	Z Tauri	5	46	54	+15 46'3
22.	U ² Andromedæ ...	1	33	1	+38 10'7	79.	U Orionis	5	50	7	+20 9'5
23.	R Piscium	1	25	41	+2 23'2	80.	Z Aurigæ	5	54	1	+53 17'5
24.	Y Andromedæ ...	1	33	59	+38 51'4	81.	R Octantis	5	55	34	-86 26'1
25.	X Cassiopeiæ ...	1	50	2	+58 47'3	82.	X Aurigæ	6	4	44	+50 15'0
26.	U Persei	1	53	12	+54 21'3	83.	η Geminorum ...	6	9	5	+22 32'1
27.	S Arietis	1	59	29	+12 4'0	84.	V Aurigæ	6	16	36	+47 42'6
28.	R Arietis	2	10	39	+24 36'6	85.	V Monocerotis ...	6	17	53	-2 8'8
29.	W Andromedæ ...	2	11	29	+43 51'6	86.	T Monocerotis ...	6	20	2	+7 8'3
30.	Z Cephei	2	13	19	+81 14'1	87.	W Geminorum ...	6	29	28	+15 24'3
31.	o Ceti	2	14	30	-3 24'8	88.	S Lynceis	6	36	17	+58 0'3
32.	R Ceti	2	21	7	-0 36'7	89.	X Geminorum ...	6	40	58	+30 22'4
33.	S Horologii ...	2	22	29	-60 0'1	90.	W Monocerotis ...	6	47	42	-7 1'9
34.	R ² Cephei	2	29	56	+80 43'4	91.	R Lynceis	6	53	23	+55 28'0
35.	R Fornacis	2	24	57	-26 31'4	92.	S ² Geminorum ...	6	55	29	+30 39'5
36.	X Eridani	2	27	36	-41 53'0	93.	Z Geminorum ...	6	58	25	+20 42'7
37.	U Ceti	2	29	7	-13 34'2	94.	R Geminorum ...	7	1	35	+22 51'2
38.	R Trianguli ...	2	31	13	+33 50'8	95.	V Canis Min. ...	7	1	46	+9 1'1
39.	Z Persei	2	33	56	+41 47'2	96.	R Canis Min. ...	7	3	26	+10 10'5
40.	T Arietis	2	42	58	+17 6'6	97.	L ² Puppis	7	10	36	-44 29'2
41.	W Persei	2	43	32	+56 35'1	98.	R ² Monocerotis ...	7	12	39	+1 16'2
42.	R Horologii ...	2	50	41	-50 16'9	99.	R Canis Maj. ...	7	15	7	-16 12'8
43.	T Horologii ...	2	57	48	-51 1'3	100.	R ² Geminorum ...	7	15	21	+31 3'6
44.	Algol	3	1	55	+40 35'2	101.	V Geminorum ...	7	17	47	+13 16'6
45.	U Arietis	3	5	44	+14 26'2	102.	U Monocerotis ...	7	26	13	-9 34'5
46.	X Ceti	3	14	32	-1 25'1	103.	S Canis Min. ...	7	27	31	+8 31'5
47.	Y Persei	3	21	11	+43 50'5	104.	Y Camelopardi ..	7	28	12	+76 16'4
48.	R Persei	3	23	56	+35 20'5	105.	Z Puppis	7	28	29	-20 27'5
49.	T Fornacis	3	25	34	-28 44'2	106.	X Puppis	7	28	37	-20 42'2
50.	U Eridani	3	46	25	-25 14'9	107.	T Canis Min. ...	7	28	40	+11 57'0
51.	T Eridani	3	51	7	-24 18'8	108.	S Volantis	7	31	25	-63 10'4
52.	λ Tauri	3	55	22	+12 13'2	109.	U Canis Min. ...	7	36	8	+8 36'3
53.	W Eridani	4	7	29	-25 22'9	110.	S Geminorum ...	7	37	17	+23 40'6
54.	R Tauri	4	23	2	+9 56'9	111.	W Puppis	7	42	47	-41 57'7
55.	S Tauri	4	23	57	+9 44'0	112.	T Geminorum ...	7	43	33	+23 58'4
56.	T Camelopardi ...	4	30	44	+65 57'3	113.	R ² Puppis	7	43	39	-41 8'5
57.	R Reticuli	4	32	33	-63 13'7	114.	U Geminorum ...	7	49	25	+22 15'3

Mean Places of Variable Stars for 1904.0 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	°			h m s	°
115.	V Puppis	7 55 29	-48 59.1	173.	R Muscæ.....	12 36 13	-68 52.8
116.	U Puppis	7 56 19	-12 34.5	174.	S Ursæ Maj. ...	12 39 45	+61 37.1
117.	S ² Puppis	8 9 23	-34 17.3	175.	U ² Virginis.....	12 42 25	+4 40.2
118.	Y Puppis	8 8 59	-34 51.1	176.	U Virginis	12 46 13	+6 4.5
119.	R Cancri.....	8 11 16	+12 1.3	177.	S Crucis	12 48 41	-57 54.6
120.	V Cancri.....	8 16 15	+17 35.4	178.	W Virginis.....	13 21 5	-2 52.9
121.	V Carinæ	8 26 46	-59 48.1	179.	V Virginis	13 22 51	-2 40.5
122.	X Carinæ	8 29 12	-58 54.5	180.	R Hydræ	13 24 28	-22 47.1
123.	U Cancri.....	8 30 17	+19 13.6	181.	S Virginis.....	13 27 59	-6 42.1
124.	T Velorum	8 34 33	-47 1.0	182.	T Ursæ Minoris.	13 32 43	+73 55.1
125.	S Cancri.....	8 38 27	+19 22.8	183.	T Centauri	13 36 16	-33 6.7
126.	R Mali	8 41 27	-27 51.1	184.	T ² Centauri ...	13 42 44	-36 23.0
127.	S Hydræ	8 48 34	+3 25.9	185.	W Hydræ	13 43 37	-27 53.2
128.	T Hydræ.....	8 51 0	-8 46.5	186.	R Canum Ven. .	13 44 50	+40 1.2
129.	T Cancri.....	8 51 11	+20 13.0	187.	X ² Centauri ...	13 45 47	-36 28.0
130.	S Mali.....	9 0 51	-24 42.3	188.	T Apodis.....	13 46 29	-77 19.7
131.	W Cancri	9 4 17	+25 38.4	189.	R ² Virginis.....	13 59 48	-8 44.3
132.	V Velorum	9 19 22	-55 33.0	190.	Z Virginis	14 5 11	-12 50.9
133.	S Antliæ	9 28 6	-28 12.3	191.	U ² Hydræ	14 6 2	-28 25.9
134.	S Velorum	9 29 36	-44 47.0	192.	R ² Centauri	14 9 39	-59 28.0
135.	U Velorum	9 29 37	-45 5.3	193.	R ² Centauri.....	14 10 12	-57 24.4
136.	R Carinæ	9 29 50	-62 21.8	194.	Y Boötis	14 17 33	+20 14.7
137.	Y Draconis.....	9 31 33	+78 17.2	195.	S Boötis	14 19 40	+54 14.8
138.	X Hydræ	9 30 55	-14 15.8	196.	S ² Virginis.....	14 22 28	+5 6.6
139.	W Ursæ Maj. ...	9 37 1	+56 23.5	197.	R Camelopardi .	14 24 48	+84 16.1
140.	R Leo. Min. ...	9 39 50	+34 57.3	198.	V Centauri	14 25 40	-56 27.7
141.	R ² Hydræ	9 40 35	-23 34.6	199.	V Boötis	14 25 53	+39 17.3
142.	R Leonis	9 42 24	-11 52.5	200.	R Boötis	14 32 58	+27 9.2
143.	l Carinæ.....	9 42 37	-62 3.9	201.	V Libræ	14 35 1	-17 14.6
144.	V Leonis	9 54 42	+21 43.2	202.	S Lupi	14 47 0	-46 13.2
145.	R ² Carinæ	9 54 58	-58 24.2	203.	U Boötis	14 49 53	+18 5.1
146.	S Carinæ.....	10 6 19	-61 4.8	204.	δ Libræ	14 55 51	-8 8.3
147.	Z Carinæ	10 10 35	-58 22.7	205.	S Apodis.....	14 59 45	-71 41.3
148.	W Velorum	10 11 40	-54 0.1	206.	T ² Libræ	15 1 0	-18 21.7
149.	R ² Velorum	10 17 58	-41 52.3	207.	T Libræ	15 5 15	-19 39.2
150.	Y Carinæ	10 29 34	-58 0.2	208.	Y Libræ	15 6 37	-5 38.9
151.	R Ursæ Maj. ...	10 37 52	+69 16.8	209.	R Triang. Austr.	15 11 10	-66 8.6
152.	S ² Hydræ	10 46 45	-28 7.4	210.	U Coronæ	15 14 17	+31 59.9
153.	V Hydræ	10 46 57	-20 44.4	211.	S Libræ	15 15 52	-20 2.5
154.	W Leonis	10 48 34	+14 13.6	212.	S Serpentis.....	15 17 10	+14 39.5
155.	U Carinæ	10 53 53	-59 13.1	213.	S Coronæ	15 17 29	+31 42.8
156.	S Leonis	11 5 53	+5 58.9	214.	S ² Libræ	15 18 43	-22 34.1
157.	S ² Centauri.....	11 16 16	-61 20.7	215.	U ² Libræ.....	15 27 54	-15 0.2
158.	Z Draconis.....	11 40 3	+72 47.7	216.	R Normæ	15 29 3	-49 11.2
159.	Z Hydræ	11 42 49	-32 44.2	217.	X Libræ	15 30 40	-20 50.7
160.	X Centauri	11 44 2	-44 13.3	218.	W Libræ.....	15 32 25	-15 51.4
161.	W Centauri	11 50 1	-58 43.1	219.	S Ursæ Minoris.	15 33 17	+78 57.5
162.	R Comæ	11 59 20	+19 19.2	220.	U Normæ	15 34 56	-55 0.1
163.	S Muscæ.....	12 7 37	-69 37.1	221.	U Libræ	15 36 27	-20 52.3
164.	T Virginis	12 9 41	-5 30.1	222.	T Normæ	15 36 40	-54 40.8
165.	R Corvi	12 14 39	-18 43.3	223.	Z Libræ	15 40 55	-20 49.3
166.	T Crucis	12 16 7	-61 44.9	224.	V Coronæ	15 46 6	+39 51.6
167.	R Crucis	12 18 21	-61 5.8	225.	B Serpentis.....	15 46 16	+15 25.5
168.	T Canum Ven. .	12 25 17	+32 2.1	226.	R Lupi	15 47 15	-36 0.7
169.	U Centauri	12 28 12	-54 7.8	227.	R Libræ	15 48 10	-15 57.1
170.	Y Virginis	12 28 55	+3 53.5	228.	R ² Libræ.....	15 50 53	-18 1.3
171.	T Ursæ Maj. ...	12 32 1	+60 0.9	229.	S Triang. Austr.	15 52 33	-63 30.2
172.	R Virginis	12 33 38	+7 31.0	230.	U Lupi	15 54 44	-29 28.9

24 *Mean Places of Variable Stars for 1904.0. [No. 339.*

Mean Places of Variable Stars for 1904.0 (continued).

No.	Star.	R.A.			Dec.	No.	Star.	R.A.			Dec.
		h	m	s	°			h	m	s	°
231.	U Triang. Austr.	15	58	46	-62 39.0	288.	U Sagittarii	18	26	14	-19 11.7
232.	Z ² Scorpii	15	58	52	-23 50.1	289.	Z ² Herculis	18	32	54	+25 58.1
233.	X Herculis	15	59	46	+47 30.1	290.	X Ophiuchi	18	33	47	+8 44.6
234.	Z Scorpii	16	0	22	-21 28.3	291.	Y Lyrae	18	34	19	+43 52.0
235.	R ² Herculis	16	1	35	+50 45.6	292.	U Coron. Austr.	18	34	34	-37 55.4
236.	R Herculis	16	1	54	+18 37.8	293.	Z ² Lyrae	18	40	3	+32 41.9
237.	X Scorpii	16	2	54	-21 16.1	294.	Y ² Lyrae	18	41	24	+34 34.3
238.	W Scorpii	16	6	9	-19 53.3	295.	W ² Lyrae	18	42	14	+43 32.2
239.	U ² Herculis	16	6	13	+25 19.3	296.	R Scuti	18	42	22	-5 48.5
240.	S Normæ	16	10	54	-57 39.8	297.	β Lyrae	18	46	32	+33 15.0
241.	W Coronæ	16	11	53	+38 3.4	298.	α Pavonis	18	47	3	-67 21.3
242.	R Scorpii	16	11	55	-22 42.5	299.	U Scuti	18	49	5	-12 43.5
243.	S Scorpii	16	11	57	-22 39.4	300.	X ² Lyrae	18	50	36	+32 42.5
244.	W Ophiuchi	16	16	14	-7 28.2	301.	R Lyrae	18	52	25	+43 49.1
245.	V Ophiuchi	16	21	23	-12 12.6	302.	R Coron. Austr.	18	55	25	-37 5.4
246.	U Herculis	16	21	33	+19 6.7	303.	Z Lyrae	18	56	8	+34 49.4
247.	Y Scorpii	16	24	3	-19 13.8	304.	T ² Lyrae	18	57	54	+37 22.7
248.	T Ophiuchi	16	28	14	-15 55.6	305.	R Aquilæ	19	1	45	+8 5.1
249.	S Ophiuchi	16	28	44	-16 57.5	306.	V Lyrae	19	5	19	+29 30.2
250.	R Aræ	16	31	46	-56 48.1	307.	X ² Sagittarii	19	8	56	-18 58.4
251.	W Herculis	16	31	49	+37 32.1	308.	U ² Lyrae	19	9	13	+41 8.5
252.	R Draconis	16	32	24	+66 57.3	309.	S Lyrae	19	9	16	+25 50.4
253.	T ³ Scorpii	16	34	28	-32 11.5	310.	S ² Lyrae	19	9	27	+33 15.0
254.	S Draconis	16	40	51	+55 6.0	311.	U Draconis	19	9	57	+67 7.3
255.	R ² Ophiuchi	16	43	25	-19 17.8	312.	W Aquilæ	19	10	13	-7 12.6
256.	S Herculis	16	47	31	+15 6.2	313.	T Sagittarii	19	10	42	-17 8.3
257.	S ² Scorpii	16	48	40	-44 56.8	314.	R Sagittarii	19	11	4	-19 28.6
258.	R ² Scorpii	16	50	31	-30 25.6	315.	V ² Lyrae	19	12	40	+32 15.1
259.	V ² Scorpii	16	51	3	-33 27.6	316.	S Sagittarii	19	13	49	-19 12.0
260.	V ² Herculis	16	56	54	+31 21.9	317.	Z Sagittarii	19	14	2	-21 6.2
261.	R Ophiuchi	17	2	15	-15 57.9	318.	U Sagittæ	19	14	36	+19 26.1
262.	T ² Herculis	17	6	56	+27 10.5	319.	U Lyrae	19	16	45	+37 42.0
263.	W ² Scorpii	17	8	34	-33 19.3	320.	T Sagittæ	19	17	25	+17 28.3
264.	U Ophiuchi	17	11	39	+1 19.0	321.	R ² Lyrae	19	22	25	+42 36.0
265.	Z Ophiuchi	17	14	40	+1 37.0	322.	U Aquilæ	19	24	11	-7 14.5
266.	S ² Herculis	17	17	41	+23 0.9	323.	V ² Cygni	19	30	0	+28 6.7
267.	S Octantis	17	27	40	-86 46.2	324.	U Vulpeculæ	19	32	26	+20 7.1
268.	U ² Scorpii	17	35	29	-43 42.1	325.	T ² Aquilæ	19	33	31	+11 30.3
269.	X Sagittarii	17	41	31	-27 47.6	326.	R Cygni	19	34	15	+49 59.1
270.	U ³ Scorpii	17	41	52	-35 40.0	327.	V ² Aquilæ	19	36	8	+9 42.0
271.	Y ² Scorpii	17	44	32	-33 40.6	328.	T Pavonis	19	39	57	-72 0.5
272.	Y Ophiuchi	17	47	30	-6 7.2	329.	T ² Cygni	19	40	56	+48 32.5
273.	S Aræ	17	51	46	-49 25.3	330.	T ³ Cygni	19	40	58	+29 2.0
274.	T ² Ophiuchi	17	51	57	+11 10.3	331.	X ² Cygni	19	42	53	+32 28.2
275.	Z Herculis	17	53	47	+15 8.4	332.	R ² Cygni	19	43	26	+48 49.9
276.	T Draconis	17	54	56	+58 13.6	333.	S Vulpeculæ	19	44	28	+27 2.8
277.	V Draconis	17	56	22	+54 52.7	334.	X Aquilæ	19	46	43	+4 13.1
278.	W Sagittarii	17	58	54	-29 35.2	335.	χ Cygni	19	46	53	+32 40.3
279.	R Pavonis	18	3	40	-63 38.1	336.	S Pavonis	19	47	7	-59 26.6
280.	T Herculis	18	5	28	+31 0.2	337.	η Aquilæ	19	47	35	+0 45.5
281.	S ² Sagittarii	18	11	14	-34 8.5	338.	R ² Sagittarii	19	49	58	-29 26.4
282.	W Lyrae	18	11	36	+36 38.4	339.	S Sagittæ	19	51	39	+16 22.7
283.	Y Sagittarii	18	15	44	-18 54.2	340.	U ² Sagittarii	19	52	7	-42 6.3
284.	V ² Sagittarii	18	21	37	-33 22.7	341.	R ² Aquilæ	19	52	37	-2 8.5
285.	d Serpentis	18	22	18	+0 8.3	342.	S ² Aquilæ	19	53	56	-8 8.6
286.	T Serpentis	18	24	8	+6 14.0	343.	Z Cygni	19	58	44	+49 46.5
287.	X ² Herculis	18	26	12	+12 32.8	344.	S Cygni	20	3	29	+57 42.6

Mean Places of Variable Stars for 1904.0 (continued).

No.	Star.	R.A.			Dec.	No.	Star.	R.A.			Dec.
		h	m	s				h	m	s	
345.	V ³ Cygni	20	3	57	+46° 1' 3	395.	X Capricorni ...	21	3	3	-21° 44' 1
346.	R Capricorni ...	20	5	55	-14 33' 3	396.	X Cephei	21	3	22	+82 40' 8
347.	S Aquilæ	20	7	12	+15 20' 1	397.	Z Capricorni ...	21	5	16	-16 34' 2
348.	W ² Aquilæ	20	7	27	+15 46' 4	398.	S ² Aquarii	21	5	57	-4 25' 1
349.	R Telescopii ...	20	7	59	-47 17' 3	399.	T Cephei	21	8	17	+68 6' 0
350.	U ² Aquilæ	20	8	14	+12 42' 4	400.	R Equulei	21	8	36	+12 24' 4
351.	W Capricorni ...	20	8	50	-22 16' 3	401.	R ² Aquarii ...	21	10	2	-3 17' 6
352.	R Sagittæ	20	9	41	+16 26' 2	402.	X Pegasi	21	16	26	+14 2' 6
353.	S ² Cygni	20	9	54	+38 26' 3	403.	T Capricorni ...	21	16	43	-15 33' 9
354.	Z Aquilæ	20	10	4	-6 26' 7	404.	S Microscopii ...	21	21	2	-30 15' 9
355.	R Delphini	20	10	17	+8 47' 9	405.	Y Capricorni ...	21	29	8	-14 24' 0
356.	T ² Sagittarii ...	20	11	21	-39 24' 5	406.	W Cygni	21	32	25	+44 56' 7
357.	V ⁵ Cygni	20	11	30	+34 12' 5	407.	S Cephei	21	36	25	+78 11' 5
358.	V Sagittæ	20	15	57	+20 48' 0	408.	U ² Cygni	21	37	24	+53 53' 4
359.	U Cygni	20	16	38	+47 35' 5	409.	R ³ Cygni	21	38	56	+43 8' 7
360.	Z ⁴ Cygni	20	19	45	+42 55' 7	410.	V ² Cygni	21	39	18	+37 34' 6
361.	W ² Cygni	20	25	21	+39 39' 6	411.	R Gruis	21	42	21	-47 21' 5
362.	U ² Capricorni ...	20	26	56	-22 0' 8	412.	T ³ Cygni	21	55	23	+43 53' 0
363.	Z Delphini	20	28	14	+17 7' 0	413.	V Pegasi	21	56	14	+5 39' 6
364.	Y ³ Cygni	20	29	46	+46 16' 4	414.	U Aquarii	21	58	5	-17 5' 4
365.	S ³ Cygni	20	30	2	+54 38' 4	415.	S Piscis Austr.	21	58	16	-28 30' 9
366.	W Delphini	20	33	18	+17 56' 9	416.	T ² Pegasi	21	59	59	+34 39' 4
367.	R Microscopii ...	20	34	12	-29 7' 8	417.	T Pegasi	22	4	12	+12 4' 2
368.	S Capricorni ...	20	36	8	-19 23' 9	418.	Y Pegasi	22	6	58	+13 53' 5
369.	V Cygni	20	38	13	+47 47' 9	419.	R Piscis Austr.	22	12	33	-30 5' 0
370.	S Delphini	20	38	39	+16 44' 6	420.	X Aquarii	22	13	23	-21 22' 8
371.	Y Aquarii	20	39	22	-5 11' 2	421.	T Gruis	22	20	5	-38 3' 2
372.	X Cygni	20	39	39	+35 14' 4	422.	S Gruis	22	20	11	-48 55' 2
373.	T Delphini	20	40	54	+16 3' 0	423.	S Lacertæ	22	24	48	+39 49' 5
374.	W Aquarii	20	41	23	-4 26' 1	424.	δ Cephei	22	25	36	+57 55' 4
375.	V Aquarii	20	41	58	+2 5' 2	425.	R Indi	22	29	11	-67 47' 0
376.	R ² Cygni	20	42	45	+44 31' 0	426.	W Cephei	22	32	48	+57 55' 9
377.	U Capricorni ...	20	42	48	-15 8' 2	427.	R Lacertæ	22	39	1	+41 52' 1
378.	V Delphini	20	43	24	+18 58' 9	428.	S Aquarii	22	51	58	-20 51' 4
379.	T Aquarii	20	44	52	-5 30' 2	429.	R Pegasi	23	1	50	+10 1' 5
380.	T Vulpeculæ ...	20	47	24	+27 53' 2	430.	V Cassiopeiæ ...	23	7	33	+59 9' 7
381.	U Pavonis	20	47	32	-63 4' 3	431.	W Pegasi	23	14	58	+25 44' 8
382.	Y Cygni	20	48	14	+34 17' 8	432.	S Pegasi	23	15	41	+8 23' 7
383.	Z ² Cygni	20	48	40	+46 59' 6	433.	R Aquarii	23	38	51	-15 49' 0
384.	S Indi	20	49	17	-54 41' 4	434.	Z Aquarii	23	47	17	-16 23' 4
385.	X Delphini	20	50	29	+17 16' 6	435.	R ² Cassiopeiæ...	23	50	53	+53 9' 8
386.	S ³ Cygni	20	52	26	+30 3' 7	436.	R Phœnicis	23	51	29	-50 19' 2
387.	W ⁵ Cygni	20	53	43	+39 48' 4	437.	V Cephei	23	51	55	+82 39' 4
388.	U ⁴ Cygni	20	56	34	+42 13' 4	438.	R Toucani	23	52	25	-65 55' 1
389.	R ² Capricorni ...	20	56	38	-27 28' 1	439.	V Ceti	23	53	0	-9 29' 7
390.	T Octantis	20	58	5	-82 29' 1	440.	U Pegasi	23	53	5	+15 25' 2
391.	R Vulpeculæ ...	21	0	7	+23 26' 4	441.	R Cassiopeiæ ...	23	53	31	+50 51' 2
392.	T ⁴ Cygni	21	1	55	+29 1' 2	442.	S Phœnicis	23	54	7	-57 6' 6
393.	V Capricorni ...	21	2	2	-24 18' 7	443.	W Ceti	23	57	12	-15 12' 6
394.	U ⁵ Cygni	21	2	29	+45 23' 5	444.	Y Cassiopeiæ ...	23	58	22	+55 8' 7

* Alternate minima only are given: the others can be readily found by adding the interval under the name of the star.

CENTAURI 198*.			S NORMÆ 240.			Y OPHIUCHI 272.			Y SAGITTÆ 283*.			κ PAVONIS 297.		
(Min. 5 ^d 11 ^h 9.			(continued).			(6 ^d 5 ^h).			(continued).			(Min. 9 ^d 2 ^h 1 ^h .		
Max. 1 ^d 13 ^h 1 ^h).			Apr.	July	Oct.	Jan.	Apr.	July	July	Sept.	Oct.	Jan.	May	Sept.
Jan.	May	Sept.	d h	d h	d h	d h	d h	d h	d h	d h	d h	Jan.	May	Sept.
11	1	1	14 23	1 23	7 12	15 14	27 7	21 22	18 3	2 7	18 12	d h	d h	d h
23	5 19	3 16		21 12	27 0				29 16	13 20	30 1	d h	d h	d h
22	16 19	14 16	May	Aug.	Nov.	Feb.	May	Aug.	Aug.	25 9	Nov.	27 15	15 17	19
22	27 19	25 16	4 11	10 0	15 12	1 17	14 10	8 1	10 5	Oct.	10 14	Feb.	2 21	8
			23 23	29 12		18 20	31 13	25 3	21 18	6 23	22 3	14 19	21 1	26
Feb.	June	Oct.	June	Sept.	5 0	Mar.	June	Sept.				Mar.	July	N
22	7 19	6 15	12 11	18 0	24 13	6 23	17 16	11 6				3 23	9 6	13
22	18 18	17 15				24 1		28 9				22 4	27 10	
21	29 18	28 15										9 8	Aug.	1
												27 12	14 15	19
Mar.	July	Nov.	V ² SCORPII 259*.			W SAGITTÆ 278*.			U SAGITTÆ 288*.			U AQUILÆ 322.		
			(Min. 6 ^d 1 ^h 5.			(Min. 7 ^d 14 ^h 1 ^h .			(Min. 6 ^d 17 ^h 9.			(Min. 7 ^d 0 ^h 1 ^h .		
			Max. 1 ^d 10 ^h).			Max. 3 ^d 0 ^h).			Max. 2 ^d 23 ^h 1 ^h).			Max. 2 ^d 4 ^h 1 ^h).		
Jan.	May	Sept.	Jan.	May	Sept.	Jan.	May	Aug.	Jan.	May	Aug.	Feb.	May	Aug.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
11	6 3	6 9	11 12	11 18	11 0	29 15	14 23	29 7	4 17	4 2	6 13	d h	d h	d h
20	1 17	11 14	23 15	23 21	23 3	30 4			18 5	4 2	6 13	d h	d h	d h
3	20 12	22 13	Mar.	July	Nov.	Feb.	June	Sept.	31 16	17 14	20 0	7 4	29 13	21
4	23 16		6 18	6 0	4 6	13 20	13 11		31 2	31 2		21 5		
			18 21	18 3	16 9	29 0	14 8	28 16	14 4	Aug.	2 12	Mar.	June	Se
			31 0	30 6	28 12	29 0	29 13		27 16	13 14	16 0	6 7	12 15	4
												20 8	26 16	18
			Apr.	Aug.	Dec.	15 5	July	Oct.				Apr.	July	3
			12 3	11 9	10 15	30 9	14 17	29 1				3 9	10 17	17
			24 6	23 12	22 18	29 22	29 22					17 10	24 18	31
			X SAGITTÆ 269*.			Y SAGITTÆ 283*.			β LYRÆ 297.			U VULPEC. 322.		
			(Min. 7 ^d 0 ^h 1 ^h .											

* Alternate minima only are given: the others can be readily found by adding the interval under the name of the star.

MAXIMA OF δ^5 CYGNI 386.

Jan.	Feb.	March.	April.	May.	June.
d h m	d h m	d h m	d h m	d h m	d h m
1 4 0	6 1 10	3 23 3	8 20 13	5 18 6	1 15 59
10 3 18	15 0 28	12 22 20	17 19 31	14 17 23	10 15 16
19 2 35	23 23 45	21 21 38	26 18 48	23 16 41	19 14 34
28 1 53		30 20 56			28 13 51
	August.			Nov.	
July.	3 11 1	Sept.	Oct.	1 3 57	Dec.
7 13 9	12 10 18	8 8 12	5 6 4	10 3 15	7 1 7
16 12 26	21 9 37	17 7 29	14 5 22	19 2 32	16 0 25
25 11 44	30 8 54	26 6 47	23 4 39	28 1 50	24 23 42

Every sixteenth maximum is given; the others may be found by adding multiples of the period $13^h 26^m.3$. The minimum takes place $1^h 55^m$ before the maximum.

MAXIMA OF υ PEGASI 440.

Jan.	Feb.	April.	May.	June.	July.
d h m	d h m	d h m	d h m	d h m	d h m
1 1 24	3 18 56	3 18 6	3 17 41	2 17 16	2 16 52
4 19 21	7 12 53	7 12 3	7 11 38	6 11 13	6 10 48
8 13 18	11 6 50	11 6 0	11 5 35	10 5 10	10 4 45
12 7 15	15 0 47	14 23 57	14 23 32	13 23 7	13 22 42
16 1 12	18 18 44	18 17 54	18 17 29	17 17 4	17 16 39
19 19 8	22 12 40	22 11 51	22 11 26	21 11 1	21 10 36
23 13 5	26 6 37	26 5 48	26 5 23	25 4 58	25 4 33
27 7 2		29 23 44	29 23 20	28 22 55	28 22 30
31 0 59					
August.	Sept.	Oct.	Nov.	Dec.	
1 16 27	4 9 59	4 9 34	3 9 9	3 8 44	
5 10 24	8 3 56	8 3 31	7 3 6	7 2 41	
9 4 20	11 21 53	11 21 28	10 21 3	10 20 38	
12 22 17	15 15 49	15 15 25	14 15 0	14 14 35	
16 16 14	19 9 46	19 9 21	18 8 57	18 8 32	
20 10 11	23 3 43	23 3 18	22 2 53	22 2 29	
24 4 8	26 21 40	26 21 15	25 20 50	25 20 25	
27 22 5	30 15 37	30 15 12	29 14 47	29 14 22	
31 16 2					

Every twentieth maximum is given; the others may be found by adding multiples of the period $4^h 29^m.8$. The minimum takes place $2^h 15^m$ after the maximum.

MAXIMA OF z^3 LYRÆ 293.

Jan.	Feb.	March.	April.	May.	June.
d h m	d h m	d h m	d h m	d h m	d h m
0 19 4	5 14 1	2 3 34	1 19 49	2 12 4	2 4 19
5 21 46	10 16 44	7 6 16	6 22 31	7 14 46	7 7 1
11 0 29	15 19 26	12 8 59	12 1 14	12 17 29	12 9 44
16 3 11	20 22 9	17 11 41	17 3 56	17 20 11	17 12 26
21 5 54	26 0 51	22 14 24	22 6 39	22 22 54	22 15 9
26 8 36		27 17 6	27 9 21	28 1 36	27 17 51
31 11 19					
July.	August.	Sept.	Oct.	Nov.	Dec.
2 20 34	2 12 49	2 5 4	2 21 19	2 13 34	3 5 49
7 23 16	7 15 31	7 7 46	8 0 1	7 16 16	8 8 31
13 1 59	12 18 14	12 10 29	13 2 44	12 18 59	13 11 14
18 4 41	17 20 56	17 13 11	18 5 26	17 21 41	18 13 56
23 7 24	22 23 39	22 15 54	23 8 9	23 0 24	23 16 39
28 10 6	28 2 21	27 18 36	28 10 51	28 3 6	28 19 21

Every tenth maximum is given; the others may be found by adding multiples of the period $12^h 16^m.2$.

MAXIMA OF γ^3 CYGNI 364.

Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h	d h
5 19	4 23	6 3	5 7	5 11	4 15	4 19	3 23	3 3	3 7	2 11	2 16
20 21	20 1	21 5	20 9	20 13	19 17	19 21	19 1	18 5	18 9	17 14	17 18

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following the Star's name are the Nos. in Catalogue, pp. 22-25.)

U CEPHEI 15.	Z PERSEI 39.	ALGOL 44.	λ TAURI 52.	R CANIS MAJ. 99.	R ² PUPPIS 115
<i>Fourth</i> ^u .	<i>Fourth</i> .	<i>Third</i> .	<i>Second</i> .	<i>Sixth</i> .	<i>Second</i> .
$i=2^d 11^h 49^m.5.$	$i=3^d 1^h 21^m.5.$	$i=2^d 20^h 49^m.$	$i=3^d 22^h 52^m.$	$i=1^d 3^h 15^m.8.$	$i=6^d 10^h 19^m.$
Jan. d h m 1 3 19 11 2 37 21 1 55 31 1 13	Jan. d h m 3 23 17 16 4 43 28 10 9	Jan. d h m 1 10 54 10 1 20 18 15 47 27 6 14	Jan. d h m 3 1 54 10 23 39 18 21 23 26 19 8	Jan. d h m 2 0 57 8 20 31 15 16 6 22 11 40 29 7 15	Jan. d h m 2 0 2 14 20 41 27 17 20
Feb. 10 0 31 19 23 49 29 23 7	Feb. 9 15 35 21 21 1	Feb. 4 20 41 13 11 8 22 1 35	Feb. 3 16 52 11 14 36 19 12 21 27 10 5	Feb. 5 2 49 11 22 24 18 17 59 25 13 33	Feb. 9 13 58 22 10 37
March. 10 22 25 20 21 43 30 21 2	March. 5 2 27 17 7 53 29 12 20	March. 1 16 2 10 6 29 18 20 56 27 11 22	March. 6 7 50 14 5 34 22 3 18 30 1 3	March. 3 9 8 10 4 42 17 0 17 23 19 52 30 15 26	March. 6 7 16 19 3 55
April. 9 20 20 19 19 38 29 18 56	April. 10 18 46 23 0 12	April. 5 1 49 13 16 16 22 6 43	April. 6 22 47 14 20 32 22 18 16 30 16 0	April. 6 11 1 13 6 35 20 2 10 26 21 45	April. 1 0 34 13 21 13 26 17 51
May. 9 18 14 19 17 32 29 16 50	May. 5 5 38 17 11 4 29 16 30	May. 5 19 56 14 10 23 23 0 50 31 15 17	May. 2 21 55 10 19 40 18 17 24 26 15 9	May. 3 17 19 10 12 54 17 8 28 24 4 3 30 23 38	May. 9 14 30 22 11 9
June. 8 16 8 18 15 26 28 14 44	June. 10 21 57 23 3 23	June. 27 5 29	June. 2 21 55 10 19 40 18 17 24 26 15 9	June. 13 6 35 20 2 10 26 21 45	June. 4 7 48 17 4 27 30 1 6
July. 8 14 2 18 13 20 28 12 38	July. 5 8 49 17 14 15 29 19 41	July. 5 19 56 14 10 23 23 0 50 31 15 17	July. 2 21 55 10 19 40 18 17 24 26 15 9	July. 3 17 19 10 12 54 17 8 28 24 4 3 30 23 38	July. 12 21 44 25 18 23
August. 7 11 56 17 11 14 27 10 32	August. 11 1 7 23 6 33	August. 9 5 44 17 20 10 26 10 37	August. 3 12 53 11 10 38 19 8 22 27 6 6	August. 7 15 2 20 11 41	August. 7 15 2 20 11 41
Sept. 6 9 50 16 9 8 26 8 26	Sept. 4 12 0 16 17 26 28 22 52	Sept. 4 1 4 12 15 31 21 5 58 29 20 25	Sept. 4 3 51 12 1 35 19 23 20 27 21 4	Sept. 1 3 11 7 22 45 14 18 20 21 13 54 28 9 29	Sept. 2 8 20 15 4 59 28 1 37
Oct. 6 7 44 16 7 2 26 6 20	Oct. 11 4 18 23 9 44	Oct. 8 10 52 17 1 19 25 15 46	Oct. 5 18 48 13 16 33 21 14 16 29 12 1	Oct. 5 5 4 12 0 38 18 20 13 25 15 47	Oct. 10 22 16 23 18 55
Nov. 5 5 39 15 4 57 25 4 15	Nov. 4 15 10 16 20 36 29 2 3	Nov. 3 6 12 11 20 39 20 1 6 29 1 3	Nov. 6 9 45 14 7 30 22 5 15 30 2 59	Nov. 1 11 22 8 6 57 15 2 31 21 22 6 28 17 40	Nov. 5 15 34 18 12 12
Dec. 5 3 33 15 2 51 25 2 9	Dec. 11 7 29 23 12 55	Dec. 7 16 0 16 6 27 24 20 54	Dec. 8 0 44 15 22 28 23 20 12 31 17 57	Dec. 5 13 15 12 8 50 19 4 24 25 23 59	Dec. 1 8 51 14 5 30 27 2 9

* The Second, Third, Fourth, or other minima are given in the columns as shown by the ordinal given at the head. Times of the other minima can be found by adding multiples of the period given.

Minima of Variable Stars of the Algol Type (continued).

V PUPPIS 115. Eighth. $1=1^d 10^h 54^m.4$.	S CANCRI 125. Fourth. $1=1^d 20^h 30^m.2$	S ANTLIS 133. Fortieth. $1=0^d 7^h 46^m.8$ $20=6 11 36^m.0$.	S VELORUM 134. Second. $1=5^d 22^h 24^m.4$.	W URSÆ MAJ. 139. Sixtieth. $1=0^d 4^h 0^m.2$ $30=5^d 0 6^m.4$.	R ² VELORUM 149 Fourth. $1=1^d 20^h 30^m.2$
Jan. d h m 2 0 8 13 15 24 25 6 39	Jan. d h m 9 10 53 18 22 31 28 10 9	Jan. d h m 1 1 46 14 0 58 27 0 10	Jan. d h m 1 7 35 13 4 24 25 1 12	Jan. d h m 0 12 1 10 12 14 20 12 27 30 12 39	Jan. d h m 1 9 7 8 19 7 16 5 7 23 15 7 31 1 7
Feb. 5 21 55 17 13 10 29 4 26	Feb. 6 21 47 16 9 24 25 21 2	Feb. 8 23 22 21 22 34	Feb. 5 22 1 17 18 50 29 15 38	Feb. 9 12 52 19 13 5 29 13 18	Feb. 7 11 7 14 21 7 22 7 8 29 17 8
March. 11 19 41 23 10 57	March. 6 8 40 15 20 18 25 7 55	March. 5 21 46 18 20 58 31 20 10	March. 12 12 27 24 9 16	March. 10 13 31 20 13 43 30 13 56	March. 8 3 8 15 13 8 22 23 8 30 9 8
April. 4 2 13 15 17 28 27 8 44	April. 3 19 33 13 7 11 22 18 49	April. 13 19 22 26 18 34	April. 5 6 4 17 2 53 28 23 42	April. 9 14 9 19 14 22 29 14 35	April. 6 19 8 14 5 8 21 15 9 29 1 9
May. 8 23 59 20 15 15	May. 2 6 26 11 18 4 21 5 42	May. 9 17 46 22 16 58	May. 10 20 31 22 17 19	May. 9 14 47 19 15 0 29 15 13	May. 6 11 9 13 21 9 21 7 9 28 17 9
June. 1 6 30 12 21 46 24 13 2	June. 30 17 20 9 4 57 18 16 35	June. 4 16 10 17 15 22 30 14 34	June. 3 14 8 15 10 57 27 7 45	June. 8 15 26 18 15 39 28 15 51	June. 5 3 9 12 13 10 19 23 10 27 9 10
July. 6 4 17 17 19 33 29 10 48	July. 28 4 13 7 15 51 17 3 28	July. 13 13 46 26 12 58	July. 9 4 34 21 1 23	July. 8 16 4 18 16 17 28 16 30	July. 4 19 10 12 5 10 19 15 10 27 1 10
August. 10 2 4 21 17 19	August. 26 15 6 2 13 37 12 1 15	August. 3 10 34 16 9 46 29 8 58	August. 1 22 12 13 19 0 25 15 49	August. 7 16 43 17 16 55 27 17 8	August. 3 11 10 10 21 11 18 7 11 25 17 11
Sept. 2 8 35 13 23 50 25 15 6	Sept. 21 12 53 1 0 30 10 12 8	Sept. 12 8 10 25 7 22	Sept. 6 12 38 18 9 26 30 6 15	Sept. 6 17 21 16 17 34 26 17 47	Sept. 2 3 11 9 13 11 16 23 11 24 9 11
Oct. 7 6 22 18 21 37 30 12 53	Oct. 19 23 46 29 11 24 7 23 1	Oct. 3 4 58 16 4 10 29 3 22	Oct. 12 3 4 23 23 52	Oct. 6 17 59 16 18 12 26 18 25	Oct. 9 5 12 16 15 12 24 1 12 31 11 12
Nov. 11 4 8 22 19 24	Nov. 16 10 39 26 22 17	Nov. 7 6 34 20 5 46	Nov. 4 20 41 16 17 30 28 14 19	Nov. 5 18 38 15 18 51 25 19 3	Nov. 7 21 12 15 7 12 22 17 12 30 3 13
Dec. 4 10 39 16 1 55 27 17 10	Dec. 6 9 55 15 21 32 25 9 10	Dec. 3 4 58 16 4 10 29 3 22	Dec. 10 11 7 22 7 56	Dec. 5 19 16 15 19 29 25 19 42	Dec. 7 13 13 14 23 13 22 9 13 29 19 18

Minima of Variable Stars of the Algol Type (continued).

Z DRACONIS 158. Sixth. 1=1 ^d 8 ^h 34 ^m .7.	δ LIBRÆ 204. Fourth. 1=2 ^d 7 ^h 51 ^m .	U CORONÆ 210. Third. 1=3 ^d 10 ^h 51 ^m .	R ARÆ 250. Second. 1=4 ^d 10 ^h 12 ^m .1.	U OPHIUCHI 264. Tenth. 1=0 ^d 20 ^h 7 ^m .7.	Z HERCULIS 2. Fourth. 1=2 ^d 1 ^h 21 ^m . 2=3 23 50. 3=6 1 10.
Jan. d h m 0 14 36 8 18 4 16 21 33 25 1 1	Jan. d h m 2 21 25 12 4 50 21 12 16 30 19 41	Jan. d h m 2 11 37 12 20 10 23 4 43	Jan. d h m 1 16 7 10 12 31 19 8 55 28 5 19	Jan. d h m 1 12 21 9 21 38 18 6 54 26 16 11	Jan. d h m 2 15 30 10 15 9 18 14 48 26 14 27
Feb. 2 4 29 10 7 58 18 11 26 26 14 54	Feb. 9 3 7 18 10 32 27 17 58	Feb. 2 13 16 12 21 49 23 6 22	Feb. 6 1 44 14 22 8 23 18 32	Feb. 4 1 28 12 10 45 20 20 2 29 5 18	Feb. 3 14 6 11 13 45 19 13 24 27 13 4
March. 5 18 23 13 21 51 22 1 19 30 4 47	March. 8 1 23 17 8 49 26 16 14	March. 4 14 55 14 23 28 25 8 1	March. 3 14 57 12 11 21 21 7 45 30 4 9	March. 8 14 35 16 23 52 25 9 9	March. 6 12 43 14 12 22 22 12 1 30 11 40
April. 7 8 16 15 11 44 23 15 12	April. 4 23 40 14 7 5 23 14 31	April. 4 16 34 15 1 7 25 9 40	April. 8 0 34 16 20 58 25 17 22	April. 2 18 26 11 3 42 19 12 59 27 22 16	April. 7 11 19 15 10 58 23 10 37
May. 1 18 41 9 22 9 18 1 37 26 5 6	May. 2 21 57 12 5 22 21 12 47 30 20 13	May. 5 18 13 16 2 46 26 11 19	May. 4 13 46 13 10 11 22 6 35 31 2 59	May. 6 7 33 14 16 50 23 2 6 31 11 23	May. 1 10 16 9 9 56 17 9 35 25 9 14
June. 3 8 34 11 12 2 19 15 30 27 18 59	June. 9 3 39 18 11 4 27 18 30	June. 16 4 25 26 12 58	June. 8 23 23 17 19 48 26 16 12	June. 8 20 40 17 5 57 25 15 14	June. 2 8 53 10 8 32 18 8 11 26 7 50
July. 5 22 27 14 1 55 22 5 24 30 8 52	July. 7 1 55 16 9 21 25 16 46	July. 17 6 4 27 14 37	July. 5 12 36 14 9 0 23 5 25	July. 2 8 15 10 17 32 19 2 49 27 12 5	July. 4 7 29 12 7 8 20 6 48 28 6 27
August. 7 12 20 15 15 49 23 19 17 31 22 45	August. 13 7 37 22 15 3 31 22 28	August. 17 7 43 27 16 16	August. 1 14 9 9 22 13 18 18 37 27 15 2	August. 4 21 22 13 6 39 21 15 56 30 1 13	August. 5 6 6 13 5 35 21 5 24 29 5 3
Sept. 9 2 13 17 5 42 25 9 10	Sept. 10 5 54 19 13 19 28 20 45	Sept. 7 0 49 17 9 22 27 17 54	Sept. 5 11 26 14 7 50 23 4 15	Sept. 7 10 30 15 19 46 24 5 3	Sept. 6 4 42 12 5 53 20 5 33 28 5 12
Oct. 3 12 38 11 16 7 19 19 35 27 23 3	Oct. 12 8 9 21 15 35 30 23 0	Oct. 8 2 27 18 11 0 28 19 33	Oct. 2 0 39 10 21 3 19 17 27 28 13 52	Oct. 2 14 20 10 23 37 19 8 54 27 18 11	Oct. 6 4 51 14 4 30 22 4 9 30 3 48
Nov. 5 2 32 13 6 0 21 9 28 29 12 56	Nov. 9 5 45 19 14 18 29 22 51	Nov. 8 4 6 18 12 39 28 21 12	Nov. 6 10 16 15 6 40 24 3 4	Nov. 5 3 27 13 12 44	Nov. 7 3 27 15 3 6 23 2 46
Dec. 7 16 25 15 19 53 23 23 21	Dec. 1 2 25 9 2 4 17 1 43 25 1 22	Dec. 1 2 25 9 2 4 17 1 43 25 1 22	Dec. 1 2 25 9 2 4 17 1 43 25 1 22	Dec. 1 2 25 9 2 4 17 1 43 25 1 22	Dec. 1 2 25 9 2 4 17 1 43 25 1 22

Minima of Variable Stars of the *Algol* Type (continued).

S ² SAGITTARI 281. Fourth.	X ² HERCULIS 287. Twelfth.	V ² LYRÆ 315. Fourth.	U SAGITTÆ 318. Fourth.	X ³ CYGNI 331. Second.	V ³ CYGNI 341. Second.
$\iota = 2^d 9^h 58^m.6.$	$\iota = 0^d 21^h 20^m.5.$	$\iota = 3^d 14^h 22^m.4.$	$\iota = 3^d 9^h 8^m.2.$	$\iota = 6^d 0^h 9^m.$	$\iota = 4^d 13^h 45^m.$
Jan. d h m 21 23 53 31 15 47	Jan. d h m 21 4 54 31 21 0	Jan. d h m 2 20 0 17 5 29 31 14 59	Jan. d h m 1 20 29 15 9 2 28 21 35	Jan. d h m 2 23 23 14 23 41 26 23 58	Jan. d h m 4 2 9 13 5 39 22 9 9 31 12 39
Feb. 10 7 42 19 23 36 29 15 30	Feb. 11 13 7 22 5 13	Feb. 15 0 28 29 9 58	Feb. 11 10 8 24 22 40	Feb. 8 0 16 20 0 34	Feb. 9 16 9 18 19 39 27 23 9
March. 10 7 25 19 23 19 29 15 14	March. 3 21 20 14 13 27 25 5 33	March. 14 19 28 29 4 57	March. 9 11 13 22 23 46	March. 3 0 51 15 1 9 27 1 26	March. 8 2 40 17 6 10 26 9 40
April. 8 7 8 17 23 3 27 14 57	April. 4 21 40 15 13 46 26 5 53	April. 12 14 27 26 23 56	April. 5 12 18 19 0 51	April. 8 1 44 20 2 2	April. 4 13 10 13 16 40 22 20 10
May. 7 6 52 16 22 46 26 14 41	May. 6 21 59 17 14 6 28 6 13	May. 11 9 26 25 18 55	May. 2 13 24 16 1 56 29 14 29	May. 2 2 19 14 2 37 26 2 54	May. 1 23 40 11 3 10 20 6 40 29 10 10
June. 5 6 35 14 22 29 24 14 24	June. 7 22 19 18 14 26 29 6 32	June. 9 4 25 23 13 55	June. 12 3 2 25 15 34	June. 7 3 12 19 3 30	June. 7 13 40 16 17 10 25 20 40
July. 4 6 18 13 22 13 23 14 7	July. 9 22 39 20 14 45 31 6 54	July. 7 23 24 22 8 54	July. 9 4 7 22 16 40	July. 1 3 47 13 4 5 25 4 22	July. 17 13 40 16 17 10 25 20 40
August. 2 6 2 11 21 56 21 13 51 31 5 45	August. 10 22 59 21 15 5	August. 5 18 23 20 3 53	August. 5 5 12 18 17 45	August. 6 4 40 18 4 58 30 5 15	August. 5 0 10 14 3 40 23 7 11
Sept. 9 21 39 19 13 34 29 5 28	Sept. 1 7 12 11 23 18 22 15 25	Sept. 3 13 22 17 22 52	Sept. 1 6 18 14 18 50 28 7 23	Sept. 11 5 33 23 5 50	Sept. 1 10 41 10 14 11 19 17 41 28 21 11
Oct. 8 21 23 18 13 17 28 5 12	Oct. 3 7 32 13 23 38 24 15 45	Oct. 2 8 22 16 17 51 31 3 21	Oct. 11 19 56 25 8 28	Oct. 5 6 8 17 6 26 29 6 43	Oct. 7 0 41 16 4 11 25 7 41
Nov. 6 21 6 16 13 1 26 4 55	Nov. 4 7 51 14 23 58 25 16 4	Nov. 13 7 49 27 17 19	Nov. 7 21 1 21 9 34	Nov. 10 7 1 22 7 18	Nov. 10 1 11 19 4 41 28 8 11
	Dec. 6 8 11 17 0 18	Dec. 4 22 6 18 10 39 31 23 12	Dec. 4 22 6 18 10 39 31 23 12	Dec. 4 7 36 16 7 53 28 8 11	Dec. 7 11 42 16 15 12 25 18 42

Minima of Variable Stars of the Algol Type (continued).

V ⁶ CYGNI 357.	Z ¹ CYGNI 360. Fourth. 1=3 ^d 10 ^h 49 ^m .2.	W DELPHINI 366. Second. 1=4 ^d 19 ^h 21 ^m .2.	U ⁵ CYGNI 394. Sixth. 1=1 ^d 11 ^h 27 ^m .0	Y CYGNI 382. Fourth. 1=1 ^d 15 ^h 8 ^m . 2=2 23 54. 3=4 15 3.	Y CYGNI 382 (con.).
Jan. d h m 4 0 30 12 10 53 20 21 16 29 7 39 Feb. 6 18 2 15 4 25 23 14 48 March. 3 1 11 11 11 34 19 21 57 28 8 20 April. 5 18 43 14 5 6 22 15 29 May. 1 1 52 9 12 15 17 22 38 26 9 1 June. 3 19 24 12 5 47 20 16 10 29 2 33 July. 7 12 56 15 23 19 24 9 42 August. 1 20 5 10 6 28 18 16 51 27 3 14 Sept. 4 13 37 13 0 0 21 10 23 29 20 46 Oct. 8 7 9 16 17 32 25 3 55 Nov. 2 14 18 11 0 41 19 11 4 27 21 27 Dec. 6 7 50 14 18 13 23 4 36 31 14 59	Jan. d h m 1 8 37 15 3 53 28 23 10 Feb. 11 18 27 25 13 44 March. 10 9 0 24 4 17 April. 6 23 34 20 18 51 May. 4 14 8 18 9 24 June. 1 4 41 14 23 58 28 19 15 July. 12 14 31 26 9 48 August. 9 5 5 23 0 22 Sept. 5 19 39 19 14 55 Oct. 3 10 12 17 5 29 31 0 46 Nov. 13 20 2 27 15 19 Dec. 11 10 36 25 5 53	Jan. d h m 2 13 11 12 3 54 21 18 36 31 9 18 Feb. 10 0 1 19 14 43 29 5 26 March. 9 20 8 19 10 50 29 1 33 April. 7 16 15 17 6 58 26 21 40 May. 6 12 22 16 3 5 25 17 47 June. 4 8 30 13 23 12 23 13 54 July. 3 4 37 12 19 19 22 10 2 August. 1 0 44 10 15 26 20 6 9 29 20 51 Sept. 8 11 34 18 2 16 27 16 58 Oct. 7 7 41 16 22 23 26 13 6 Nov. 5 3 48 14 18 30 24 9 13 Dec. 3 23 55 13 14 38 23 5 20 32 20 3	Jan. d h m 0 20 29 9 17 10 18 13 52 27 10 34 Feb. 5 7 16 14 3 57 23 0 39 March. 2 21 21 11 18 2 20 14 44 29 11 26 April. 7 8 8 16 4 49 25 1 31 May. 3 22 13 12 18 55 21 15 36 30 12 18 June. 8 9 0 17 5 41 29 2 23 July. 4 23 5 13 19 47 22 16 28 31 13 10 August. 9 9 52 18 6 33 27 3 15 Sept. 4 23 57 13 20 39 22 17 20 Oct. 1 14 2 10 10 44 19 7 25 28 4 7 Nov. 6 0 49 14 21 31 23 18 12 Dec. 2 14 54 11 11 36 20 8 18 29 4 59	Jan. d h m 1 18 23 7 18 12 13 18 2 19 17 52 25 17 42 31 17 32 Feb. 6 17 22 12 17 12 18 17 2 24 16 51 March. 1 16 40 7 16 30 13 16 19 19 16 9 25 15 59 31 15 49 April. 6 15 38 12 15 28 18 15 18 24 15 8 30 14 58 May. 6 14 47 12 14 37 18 14 27 24 14 16 30 14 6 June. 5 13 56 11 13 46 17 13 35 23 13 25 29 13 15 July. 5 13 5 11 12 54 17 12 44 23 12 34 29 12 23 August. 4 12 13 10 12 3 16 11 53 22 11 42 28 11 32 Sept. 3 11 22 9 11 12 15 11 1 21 10 51 27 10 41 Oct. 3 10 31 9 10 20 15 10 10 21 10 0 27 9 50	Nov. d h m 2 9 39 8 9 29 14 9 19 20 9 9 26 8 58 Dec. 2 8 48 8 8 38 14 8 28 20 8 18 26 8 7 32 7 57 T ³ CYGNI 412. Jan. d h m 15 11 29 Feb. 15 18 46 March. 18 2 4 April. 18 9 22 May. 19 16 40 June. 19 23 57 July. 21 7 15 August. 21 14 33 Sept. 21 21 51 Oct. 23 15 9 Nov. 23 12 26 Dec. 24 19 44

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to the Catalogue on pp. 22-25; M signifies maximum; m, minimum.

January.	January (con.).	February (con.).	March (con.).	March (con.).	April (con.).	May (con.).	June.	July (con.).
No. d	No. d	No. d	No. d	No. d	No. d	No. d	No. d	No. d
443 M 20	47 M	301 m 11	184 M 3	387 M 24	437 m 15	406 M 6	46 M 1	46 M 24
421 m	155 m	333 m	148 M	367 M 16	26 m	383 M	171 M	15
175 M	367 m	102 M 12	9 M 4	347 m 17	405 M	143 M 7	56 m	25 33
30 M	143 M 21	183 M	387 M 25	142 m	206 M	218 M 2	75 M	26 28
333 M	10 m	355 m	431 M	117 m	342 M 8	398 M	271 m	30
238 m	25 M 22	143 m	181 m 26	135 M 18	151 m	344 M 4	105 M	40
385 M	43 M	387 M 13	94 m	67 M 19	423 M	397 m	347 M	8
103 M	200 m	64 M 5	12 M	80 M	257 M 9	276 M	96 M	27 11
79 m	23 280 m	90 m	155 M	302 M	68 M	172 m	119 m	9
395 M	24 135 M	117 m	301 M 27	123 M	333 M 10	442 M 5	156 M	28 30
300 M	13 M	254 m 14	433 M	70 m 20	301 M 11	24 M	359 M	29
117 m	387 M 15	154 M 6	6 M 28	246 M	48 M	138 M 6	301 M	13
148 m	25 352 M	171 m 7	243 M	159 m	97 m	391 M	159 m	19
327 M	102 m	302 m	402 M 29	102 M	290 m 12	183 M	328 m	18
387 M	127 M 26	111 M 16	7 m	301 m 21	217 m 13	271 m 7	47 m	29 15
341 M	153 M 17	251 M 8	224 M	183 m	256 m 14	88 M	367 m	5
196 M	96 m	203 M	226 m 30	31 M 22	271 m	301 m 8	374 M	30
73 M	320 M	418 M 9	223 M	136 m 23	338 m 15	102 M	271 M	8
393 m	28 144 M	258 m	333 M	197 m	111 m	36 M 10	211 M	30 27
32 m	241 M 18	110 m	5 m		228 M 24	351 M	277 M	10
88 m	217 M	351 m 10	3 M		143 m	415 M	143 M	
276 m	408 m 19	4 M	391 m	April.	146 m	333 M 12	387 M	
277 m	334 M 29	20 M 11	350 M	1 329 M 25	37 M	302 m	102 m	Jul
325 M	97 M	172 M	180 M	309 M	316 m 16	62 M	314 m	40
179 M	270 M 30	55 M	34 M	143 M 26	189 M	384 M 13	403 M	32
419 M	24 m	271 m	102 m 2	58 M 27	168 M	155 m	103 m	2 38
143 M	150 m	165 M 12	358 M	271 m	430 m 17	428 M 14	352 M	25
33 M	96 M 31	92 M 23	65 m 3	438 M	135 m	282 M 15	176 M	32
444 M	145 M	23 m 13	271 m 5	352 M	102 m	117 M	131 M	9
85 M		219 M 24	225 m	129 m	292 m	22 m	232 M	22
128 M	February.	117 M 14	381 M	340 m 28	361 m 18	203 m	28 M	
347 M	1 271 m	62 m	390 M 6	169 M 29	271 M	222 m	335 m	4 16
352 m	2 292 M 25	441 M	274 M	117 M	161 m	261 m 16	267 M	
160 M	237 M	143 M	244 M	343 m	279 m 19	14 M	358 M	5 43
81 M	410 m	112 M 15	379 M	19 m 30	10 M	43 m	111 M	6 28
271 m	282 m	421 M	27 M	140 m	421 m	20 m	117 m	40
232 M	3 41 M	442 m	32 M 7	377 M	323 m 20	271 M 17	235 M	7 14
201 M	407 M	135 m	354 m 8	155 m	265 m 21	155 M	69 M	14
174 m	4 59 M	401 m 16	262 M	253 M	195 m	320 m	427 M	37
214 M	49 M 26	54 M	289 M	311 M		51 M 18	97 M	9 50
328 M	5 159 M	170 M	397 M	8 M	May.	361 M 23	171 M	21
117 M	415 M	406 m	28 m 9	29 M 1	191 M	5 M 19	385 m	0 10
8 m	434 M 27	393 M	18 m	271 M	55 M	353 m 20	266 M	7
45 M	396 M 28	271 M 17	312 M	404 m	157 m 24	207 M	401 M	30
48 m	40 M	252 M	356 m 10	280 M	2 49 M 25	201 m	32 m	17
215 M	157 M	155 m 19	143 m	199 m	200 M 26	238 M	25 m	18
370 M	271 M 29	120 M	232 m 11	378 M	3 327 M	79 M 21	280 M	40
403 m	435 M	187 M	176 m	399 M	387 M	354 M	116 M	11 22
354 M	242 m	266 m 20	375 M	430 m	369 M	84 M	332 M	30
175 m	9 61 M	429 m	271 M 12	107 M	130 M	2 135 M	260 m	21
432 M	77 M		416 M 13	155 M	38 m 28	185 m	425 M	12 40
271 M	146 M		422 M	387 M	4 305 M 29	143 m	90 M	13 12
301 M	314 M		245 M 14	319 M	89 M	352 m 23	271 m	27
159 m	207 m	1 46 m	352 m	214 m	109 m 30	420 m	370 m	14 2
11 M	213 M	2 267 m	186 m 21	159 M	5 174 M 31	30 M	159 M	16 10
390 m	268 M	2 211 m	260 M	101 M	346 M	242 M	302 M	17
81 M	74 m	161 m 22	249 M	395 m	117 m	161 M	110 M	17 1

Maxima and Minima of Variable Stars (continued).

July (con.).	August (con.).	August (con.).	September (con.).	September (con.).	October (con.).	November (con.).	November (con.).	Dec (con.).
d No.	d No.	d No.	d No.	d No.	d No.	d No.	d No.	d No.
18 61 M	7 254 M	24 352 M	10 413 M	30 301 m	17 117 m	4 314 M	27 120 M	14
42 m	155 M	390 m	211 m		18 390 M	202 M	8 117 m	
20 362 M	389 M	363 M	397 M	October.	155 m	5 97 M	9 435 M	
271 M	117 M	216 M	383 m		143 m	404 m	135 M	
340 M	352 m	436 M	4 m	1 257 m	19 370 M	7 184 M	326 M	17
157 M	143 m	46 m	102 m	2 217 m	20 80 M	70 m	189 M	
251 m	271 M	43 M	271 m	172 m	21 410 M	333 m	218 M	
27 m	120 m	41 m	375 m	2 18 M	31 m	8 20 m	30 432 M	18
160 m	308 M	89 m	143 m	3 385 M	22 76 M	9 405 M	354 m	
21 23 M	305 M	222 M	351 m	170 M	23 141 M	183 M		19
414 M	367 M	303 M	328 M	354 M	301 M	169 m	December.	
334 m	20 M	32 M	406 M	379 M	155 M	302 m		
22 387 M	40 m	135 m	421 m	271 m	10 m	10 12 M	1 155 M	
333 M	238 m	54 m	304 m	138 m	271 m	11 159 M	312 m	
301 M	183 M	271 M	155 M	145 m	180 m	13 137 M	2 150 M	20
23 354 m	387 M	38 M	179 M	4 329 M	24 128 M	249 M	2 M	
24 205 M	225 M	206 m	317 M	207 m	159 m	12 271 m	3 282 M	
391 m	73 m	327 M	168 m	5 353 M	367 m	13 137 M	325 M	21
245 m	12 16 M	243 M	117 M	433 m	25 49 M	15 375 M	146 M	
25 356 M	302 m	387 M	256 M	444 m	26 109 M	102 M	271 m	22
77 m	62 m		262 m	6 270 M	88 m	203 m	340 m	23
26 408 M	47 m	September.	28 m	26 M	27 82 M	406 m	4 335 M	
169 m	13 71 M	19 241 M	127 M	61 m	8 M	16 48 M	33 M	
27 117 m	316 M	208 M	271 M	59 m	28 53 M	301 m	349 M	24
28 135 M	15 430 M	439 M	387 M	266 m	53 M	267 m	333 M	
371 M	102 M	58 M	358 M	261 m	102 m	17 416 M	32 m	25
102 m	294 M	247 M	236 M	7 261 M	29 377 M	232 M	5 81 M	
29 49 M	159 m	50 M	146 m	119 M	347 M	91 M	6 143 M	
74 M	301 m	200 m	242 m	290 M	30 M	359 m	185 M	26
442 m	16 142 M	333 m	302 M	260 M	117 M	18 160 M	85 M	
405 m	373 M	159 M	292 m	8 11 M	6 m	19 355 m	7 66 M	27
30 7 M	279 M	95 M	280 M	234 M	226 m	387 M	391 m	
197 M	203 M	401 m	161 m	9 182 M	30 271 M	271 M	8 400 M	
64 m	17 37 m	3 195 M	23 140 M	10 271 M	387 M	246 m	423 M	
	18 347 m	4 24 m	24 239 M	65 M	135 m	20 78 M	301 M	
August.	19 429 M	5 148 M	201 M	387 M	171 m	21 437 M	110 m	28
1 188 M	237 M	6 418 M	391 M	311 m	31 143 M	214 m	9 351 M	
155 m	22 M	212 M	25 144 M	92 M	252 M	395 m	172 M	29
2 17 M	67 m	411 M	190 M	403 m	344 m	22 350 M	60 M	
393 M	3 m	186 M	143 M	12 442 M	November.	292 M	398 M	
9 m	282 m	301 M	310 M	265 M	23 56 M	251 M	156 M	
393 m	98 M	101 m	183 m	69 m	1 90 m	215 M	387 M	
271 m	255 M	117 m	402 M	176 m	441 m	143 m	117 M	30
136 M	143 M	97 m	391 M	157 m	2 62 M	24 103 M	254 m	
151 M	232 M	175 m	135 M	213 m	352 M	25 14 m	11 112 M	31
8 m	214 M	434 M	248 M	14 431 M	397 m	26 46 M	200 M	
5 417 M	111 m	153 M	407 m	15 111 M	320 m	309 m	12 316 m	32
156 m	23 271 m	300 M	152 M	16 277 m	5 m	309 m	15 430 m	
174 m	336 m	155 m	47 M	17 425 m	3 415 M	258 m	132 m	
	174 m	224 m	102 M	352 m	305 m		14 361 M	
		338 M	323 M					

DOUBLE STARS.

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos- angle.	Dis- tance.	Epoch. 1900+.	Ob- server.
		h m	° ' "					
1.	Σ 3062	0 0'9	+57 52	6½; 7½	345°0	1'46	1'75	B.
2.	Ω 12, λ Cassiop.	0 26'4	+53 58	6½; 6½	151°6	0'47	2'88	B.
3.	β 395	0 32'2	-25 19	6'5; 6'7	293°6	0'57	2'80	D.
4.	β 495	0 43'4	+18 9	8; 8	222°3	0'54	2'85	W.B.
5.	β 866	0 39'1	+42 34	9; 9	69°1	1'70	1'75	B.
6.	Σ 60, η Cassiop.	0 43'0	+57 17	3½; 7½	226°4	5'68	3'19	M.
7.	Ω 20, 66 Piscium	0 49'2	+18 29	6; 6	315°0	0'43	3'54	W.B.
8.	Σ 73, 36 Andromedæ ..	0 50'1	+23 2	6; 6½	22°6	1'08	2'83	W.B.
9.	Σ 113, 42 Ceti	1 14'3	-1 3	6½; 7	352°4	1'25	2'99	W.B.
10.	Σ 138	1 30'8	+7 7	7; 7½	40°1	1'47	3'45	W.B.
11.	ρ Eridani	1 35'7	-56 42	6; 6	223°0	8'12	1'05	T.
12.	Ω 38, γ Androm. BC.	1 57'8	+41 51	5½; 6	113°8	0'45	3'81	Ls.
13.	Σ 205, γ Androm. AB.	1 57'8	+41 51	2½; 5½	61°8	10'20	3'81	Ls.
14.	Σ 228	2 7'3	+56 59	6½; 7½	90°6	0'52	2'11	Ls.
15.	Σ 305	2 41'8	+18 58	7½; 8	315°8	3'06	2'47	W.B.
16.	Σ 333, ϵ Arietis	2 53'5	+20 56	5½; 6	206°8	1'13	2'06	Ls.
17.	β 525	2 53'5	+21 13	7½; 7½	143°3	0'32	2'38	D.
18.	β 1030	3 4'1	+21 20	8½; 8½	159°9	0'53	2'11	Ls.
19.	Ω 53	3 11'3	+33 16	7; 8	228°4	0'64	3'20	Ls.
20.	Σ 412, 7 Tauri	3 28'5	+24 8	6½; 7	13°8	0'17	0'10	Ls.
21.	Σ 425	3 33'8	+33 49	7½; 8	269°4	2'63	2'09	W.B.
22.	Ω 531	4 0'8	+37 49	6½; 8½	134°4	1'69	3'20	Ls.
23.	Ω 82	4 17'1	+14 49	7; 9	117°7	0'66	1'57	D.
24.	β 883, AB	4 45'6	+10 54	7½; 8	90°9	0'24	3'05	Ls.
25.	" AC	"	"	7½; 10	157°6	17'60	3'06	Ls.
26.	Σ 535	4 17'7	+11 9	7; 8	324°4	1'47	2'07	W.B.
27.	Aldebaran	4 30'2	+16 18	1; 13½	109°3	31'04	0'42	Ls.
28.	Ω 98, ι Orionis	5 2'4	+8 23	6; 6½	164°6	0'63	3'04	W.B.
29.	Σ 742, 380 Tauri	5 30'4	+21 56	7; 7½	259°6	3'33	0'20	M.
30.	Σ 749	5 30'9	+26 53	6½; 7½	165°9	0'72	2'07	W.B.
31.	Σ 932	6 28'5	+14 49	8; 8	324°6	1'94	0'10	Ls.
32.	Ω 149	6 30'0	+27 22	6½; 9	275°9	0'64	3'17	Ls.
33.	Σ 948, 12 Lynceis AB.	6 37'3	+59 33	6; 6½	117°2	1'51	2'22	M.
34.	Sirius	6 40'6	-16 34	1; 10	127°9	6'31	3'12	D.
35.	Ω 156	6 41'6	+18 18	6½; 7	298°3	0'50	3'12	W.B.
36.	Σ 1037	7 6'6	+27 24	7; 7	300°0	0'75	2'22	Ls.
37.	Ω 170	7 12'2	+9 29	7; 7½	107°0	1'18	3'23	Ls.
38.	Σ 1066, δ Geminor. ...	7 14'1	+22 10	3; 8	211°0	7'44	1'23	W.B.
39.	β 579	7 27'9	+33 20	8; 10½	218°7	0'95	1'32	Ls.
40.	Ω 175	7 28'7	+31 11	6; 6½	325°7	0'54	2'27	Ls.
41.	Σ 1110, Castor	7 28'2	+32 7	2; 3	223°0	5'68	2'27	Ls.
42.	Procyon	7 34'0	+5 29	1; 8½	314°9	5'39	2'21	Ls.
43.	Σ 1187	8 3'2	+32 31	7; 8	224°5	1'88	2'26	W.B.
44.	Σ 1196, ζ Cancri AB.	8 6'4	+17 59	5½; 7½	359°2	1'16	2'25	W.B.
45.	" " AC	"	"	5½; 6½	112°5	5'49	2'25	W.B.
46.	" " BC	"	"	7½; 6½	126°0	5'86	2'25	W.B.
47.	β 205	8 28'8	-24 16	7; 7	230°5	0'70	3'44	D.
48.	β 208	8 34'6	-22 20	7; 8	116°8	0'20	1'26	Ait.
49.	Σ 1273, ϵ Hydrae AB.	"	"	4; 6	23°0	0'13	2'24	Ls.
50.	Σ 1273, ϵ Hydrae AC.	8 41'5	+6 50	4; 7	234°1	3'47	2'22	Ls.
51.	Σ 3121	9 11'9	+29 0	7½; 7½	21°6	0'75	1'27	Ls.
52.	Ω 201	9 18'0	+28 20	7; 11	221°2	1'52	3'18	W.B.
53.	Σ 1348	9 19'2	+6 50	7½; 7½	319°9	1'80	0'31	Ls.
54.	Σ 1356, ω Leonis	9 23'1	+9 30	5½; 7	111°9	0'68	2'21	B.
55.	Σ 1389	9 45'3	+27 29	8; 9	313°0	2'15	1'31	Ls.
56.	A. C. 5, 8 Sextantis	9 47'5	-7 37	5½; 6	80°3	0'38	2'70	B.
57.	Ω 215, P. x. 23	10 10'9	+18 14	6½; 7	210°2	0'88	2'53	D.
58.	Σ 1424, γ Leonis	10 14'4	+20 22	2; 4	115°7	3'81	2'29	Ls.
59.	Ω 218	10 22'1	+4 5	7; 9	75°5	1'09	3'29	Ls.
60.	Ω 224	10 34'5	+9 22	7; 9	300°7	0'59	3'27	Ls.

DOUBLE STARS (continued).

No.	Star's name.	R. A. 1900.	Dec. 1900.	Mag.	Pos.- angle.	Dis- tance.	Epoch 1900+.	Ob- server.
		h m						
61.	$\text{O}\Sigma 229$	10 42.3	+41 38	6 ; 7	322.3	0.78	2.36	Is.
62.	$\Sigma 1523$, ξ Ursæ Maj.	11 12.9	+32 6	4 ; 5	144.0	2.27	2.35	W.B.
63.	$\Sigma 1536$, ϵ Leonis	11 18.7	+11 5	4½ ; 7½	53.2	2.17	0.43	M.
64.	$\text{O}\Sigma 235$	11 26.7	+61 38	6 ; 7½	120.9	0.58	0.43	B.
65.	$\beta 603$	11 53.5	+14 50	6½ ; 10	320.3	0.84	2.31	Is.
66.	$\Sigma 1647$, 191 Virginis.....	12 25.5	+10 16	7½ ; 8	220.1	1.26	1.39	Is.
67.	$\Sigma 1670$, γ Virginis	12 36.7	— 0 54	3 ; 3	328.1	5.74	2.40	W.B.
68.	$\Sigma 1687$, 35 Comæ.....	12 48.1	+21 48	5 ; 8	80.5	1.04	2.32	M.
69.	$\beta 112$	12 55.2	+19 1	9 ; 9½	293.8	1.92	2.32	Is.
70.	$\text{O}\Sigma 266$	13 23.6	+16 14	7 ; 8	341.8	1.63	3.33	Is.
71.	$\beta 933$	13 30.0	+33 39	8 ; 8½	29.5	2.32	1.37	Ait.
72.	$\Sigma 1768$, 25 Can. Ven.	13 33.2	+36 48	6 ; 8	133.2	1.28	3.49	D.
73.	$\Sigma 1785$	13 44.5	+27 29	7½ ; 8	289.2	1.45	2.57	M.
74.	$\text{O}\Sigma 278$	14 8.3	+44 39	7½ ; 7½	90.2	0.40	1.50	Is.
75.	$\Sigma 1819$	14 10.3	+ 3 36	7½ ; 7½	179.7	1.45	0.47	M.
76.	α Centauri.....	14 32.8	— 60 25	1 ; 1	210.7	22.01	1.33	T.
77.	$\Sigma 1865$, ζ Boötis	14 36.3	+14 10	4 ; 4	149.6	0.32	2.34	Is.
78.	$\Sigma 1877$, ϵ Boötis	14 40.6	+27 30	3 ; 6½	328.4	2.65	0.53	M.
79.	$\Sigma 1879$	14 41.3	+10 4	8 ; 8½	132.7	0.46	2.31	Is.
80.	$\Sigma 1888$, ξ Boötis	14 46.7	+19 31	5 ; 7	186.9	2.36	3.47	M.
81.	$\beta 31$	14 47.8	+19 9	8½ ; 9½	197.4	1.39	1.34	Is.
82.	$\text{O}\Sigma 287$	14 47.9	+45 20	7½ ; 7½	322.5	0.71	3.35	Is.
83.	$\Sigma 1932$, 1 Cor. Bor.	15 14.0	+27 13	6½ ; 7	150.8	0.84	0.55	M.
84.	$\Sigma 1937$, η Cor. Bor.	15 19.1	+30 40	6 ; 6	17.2	0.93	3.64	M.
85.	$\Sigma 1938$, μ^2 Boötis.....	15 20.7	+37 22	6½ ; 7½	68.1	0.99	3.62	M.
86.	$\text{O}\Sigma 298$	15 32.4	+40 8	7 ; 7½	185.5	1.17	3.35	Is.
87.	$\Sigma 1967$, γ Cor. Bor.	15 38.5	+26 36	4 ; 7	110.6	0.54	1.42	Is.
88.	$\text{O}\Sigma 303$	15 56.2	+13 33	7 ; 7½	141.6	0.72	2.72	Is.
89.	$\Sigma 1998$, ξ Scorpïi	15 58.8	— 11 5	5 ; 5½	242.2	0.50	1.46	Ait.
90.	$\Sigma 2032$, σ Coronæ.....	16 11.1	+34 7	6 ; 6½	210.3	4.38	0.72	Is.
91.	$\Sigma 2055$, λ Ophiuchi	16 25.8	+ 2 12	4½ ; 5½	60.2	1.22	2.40	Is.
92.	$\Sigma 2084$, ζ Herculis	16 37.5	+31 47	3 ; 6½	205.6	1.04	2.48	M.
93.	$\Sigma 2114$	16 56.5	+ 8 36	6½ ; 8	160.1	1.26	1.51	B.
94.	$\Sigma 2140$, α Herculis	17 10.1	+14 30	2½ ; 6	112.1	4.88	1.52	W.B.
95.	$\beta 629$	17 15.0	+32 10	8½ ; 8½	344.1	1.15	1.43	Is.
96.	$\Sigma 2173$, 221 Ophiuchi	17 25.5	— 0 59	6 ; 6	326.8	1.19	3.41	D.
97.	$\Sigma 2262$, τ Ophiuchi	17 57.6	— 8 11	5 ; 6	260.0	1.68	3.74	M.
98.	$\Sigma 2272$, 70 Ophiuchi	18 0.4	+ 2 32	4 ; 6	198.3	1.68	3.63	M.
99.	$\text{O}\Sigma 358$	18 31.3	+16 54	6½ ; 7	192.0	1.82	3.50	W.B.
100.	$\beta 648$	18 54.2	+23 39	6 ; 9	220.0	1.11	1.61	Is.
101.	γ Coronæ Aust.....	19 0.0	— 37 12	5½ ; 5½	143.3	1.93	1.40	T.
102.	$\Sigma 2525$	19 22.9	+27 8	7½ ; 7½	313.3	0.45	2.67	Is.
103.	$\Sigma 2579$, δ Cygni	19 41.8	+44 53	3 ; 8	297.8	1.68	3.01	M.
104.	$\Sigma 2695$	20 27.7	+25 28	6 ; 8	80.6	1.11	1.60	Is.
105.	$\beta 151$, β Delphini	20 32.7	+14 15	3½ ; 5½	19.8	0.40	2.68	Is.
106.	$\Sigma 2744$	20 57.8	+ 1 9	6½ ; 7½	163.0	1.33	1.72	Is.
107.	$\Sigma 2799$	21 23.9	+10 38	7 ; 7½	118.0	1.47	1.84	W.B.
108.	$\Sigma 2822$, μ Cygni	21 39.7	+28 18	4 ; 5	122.1	2.60	1.86	Is.
109.	$\Sigma 2878$	22 9.6	+ 7 29	6½ ; 9	124.2	1.11	1.81	B.
110.	$\beta 172$	22 19.0	— 5 21	6 ; 6	7.4	0.73	2.30	D.
111.	$\text{O}\Sigma 483$, 52 Pegasi.....	22 54.2	+11 12	6 ; 7½	224.7	1.05	2.90	Is.
112.	$\beta 79$	23 12.5	— 2 4	8 ; 8½	84.8	0.76	1.78	Ait.
113.	$\beta 80$	23 13.2	+ 4 53	8 ; 8½	9.1	0.34	1.78	Ait.
114.	$\beta 720$	23 29.0	+30 46	6 ; 6	163.8	0.30	1.37	Is.
115.	$\Sigma 3050$	23 54.6	+33 10	6 ; 6½	214.4	2.55	1.88	W.B.

In the column headed "Observer," the following abbreviations are used:—

L. Leavenworth.
Is. Lewis.
 β . Burnham.

M. Maw.
Ait. Aitken.
T. Tebbutt.

B. Bryant.
Bd. Barnard.

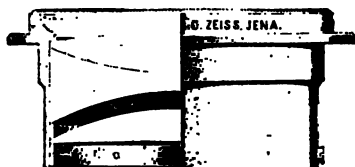
W.B. Bowyer.
D. Doolittle.

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A MONTHLY REVIEW OF ASTRONOMY

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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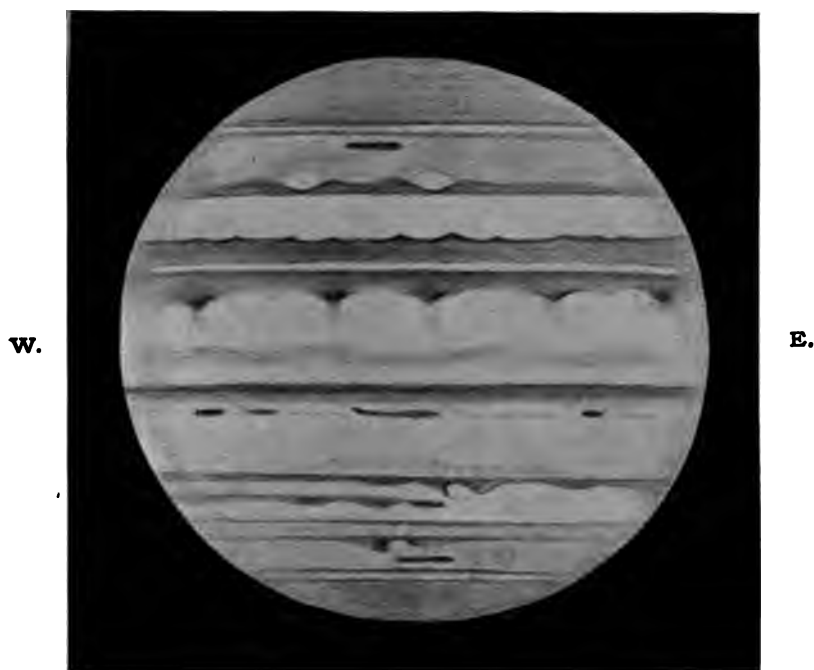
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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVII.

JANUARY, 1904.

No. 340.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1903 December 11.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

Mr. Whittaker read the Minutes of the Meeting held on Friday, November 13th, 1903, which were confirmed.

Mr. Whittaker. Fifty-six presents have been received since the last meeting, including, amongst others, A. Auwers, 'Neue Reduction der Bradley'schen Beobachtungen,' Band i., presented by Prof. Auwers; 'Carte photographique du Ciel' (23 charts), and fasc. 7 of the 'Atlas photographique de la Lune' by MM. Lœwy and Puiseux, presented by the French Minister of Public Instruction; four original negatives of the Moon, taken with the Equatorial Coudé of the Paris Observatory, presented by M. Puiseux; photograph of Langrenus's map of the Moon, presented by the Paris Observatory; portrait of Sir R. S. Ball (enlarged photograph), presented by Sir R. S. Ball.

The President. I am sure you will be pleased to hear that a portrait of Sir Robert Ball has been added to our picture gallery of Presidents to which I referred at last Meeting. I am glad to say that owing to a suggestion of Mr. Lynn we are on the eve of getting a portrait of Colebrooke—the gentleman who presided at the first Meeting of the Society when Sir William Herschel was not present. I hope the collection will ultimately become quite a complete one. We still want a portrait of Mr. George Bishop, and if any Fellow can suggest where we can procure one we shall feel indebted. There is also a little map of the Moon presented by the Paris Observatory, about which Mr. Saunder will have a

word to say presently. The meeting will no doubt return their thanks to the donors of the presents. (Hear, hear.)

The following gentlemen were elected Auditors of the Accounts for the current year :—Mr. Crommelin, Dr. Lockyer, and Mr. Thwaites.

The President. May I just make a remark now that I think is *à propos* of the list of presents? Mr. Ritchey has sent over to the Society two original negatives of nebulae. It will be in the memory of Fellows that a little discussion took place as to the process and as to the issuing of photographs which have been developed in something not quite the usual way, and in order to show that the original negatives are accurately reproduced, Mr. Ritchey has sent over two of them which are exhibited in the library, and he would be very glad if Fellows would compare these with reproductions which have been issued more or less for pictorial purposes and see if they can detect any difference of the kind some imagine. I hope that the opportunity will be freely taken to make this comparison. I may say that some of us examined the negatives, and we have been quite unable to find anything on the copies which is not on the original negatives, or *vice versa*. It may be that at the next Meeting we may discuss the whole question of reproduction of photographs; and if so, no doubt it will form an interesting subject. I may also at the present moment refer to the presence of the large instrument now on view in this room, the Stereocomparator. This is a present to the Oxford University Observatory from Mr. C. L. Brook, a Fellow of the Society. He very generously offered to present such an instrument to the Observatory if we were clear it would help us in our work. From experiments made by Dr. Max Wolf, even though we have réseaux on the plates and two or three images of every star on each plate, we can use it with great advantage. That was a point on which there was some doubt, but a paper will appear in the 'Monthly Notices' by Dr. Max Wolf which should be, I think, of interest. Mr. Brook has presented it to the University of Oxford, and we hope to do good work with it. I thought the Fellows would like to see it before it went to Oxford. It will be left for a few days longer, so that those who have not time to examine it now will be able to do so at their leisure. Now, as to the communications received, perhaps, Dr. Rambaut will give us his paper on the Moon.

Dr. Rambaut then read a paper on, and showed pictures of, two drawings of the Mare Serenitatis by John Russell, R.A., affording some hitherto unpublished evidence as to the appearance of Linné in the year 1788. He said :—In the fifty-sixth volume of the 'Monthly Notices' there appeared a paper by the late Mr. Stone entitled "Note on a Crayon Drawing of the Moon by John Russell, etc." In that paper a passing reference was made to a number of detailed pencil-drawings by the same artist of different portions of the Moon's surface, which are also in the

possession of the Radcliffe Trustees. We have also a very beautiful drawing of Russell's, representing the full Moon, which I do not think has ever been published; but neither in this nor in the picture of the gibbous Moon, which is reproduced in Mr. Stone's paper, is there any trace of the crater Linné.

Russell's drawings, which were for the most part made on separate sheets of paper, are all contained in a large folio volume presented by his grandson in 1873. They have been very carefully mounted, and although now more than 100 years old, they are still in a very good state of preservation.

Many of the drawings have memoranda in Byrom's system of shorthand written upon them, giving the date on which the drawing was made, but unfortunately this particular was sometimes omitted.

From a memorandum left by Prof. Rigaud we find that Russell was provided with a six-foot reflector made by Dr. Herschel, but that he more frequently observed with an achromatic telescope (aperture not given), and that he always preferred a low power in examining the Moon.

Out of 187 drawings it is found that only two represent the region containing Linné. The first of these, now on the screen, is particularly interesting, having been made on the 12th of April, 1788. It was thus executed in the same year as Schroeter's drawing and at very nearly the same phase of the Moon (*viz.*, 7 days). On this drawing Linné is represented as a small white spot without any indication of a crater-like depression, and much resembles its appearance at the present day as seen with a small telescope or on a photograph.

The second drawing [Screen], though undated, probably belongs to the same year (1788). Linné is here represented as a conspicuous white spot with perhaps a slight shading to the south. There is no indication of a crater; but this is not to be wondered at, as the illumination was almost vertical at the time. A remarkable feature of the drawing is the relatively large size of Linné, which is represented as more conspicuous than Bessel.

Studies of Mersenius and Copernicus—both of which have been suspected of change—are also included amongst Russell's drawings.

Mr. Saunder. The Society has now the copy of a photograph of a map of the Moon which is in the National Library at Paris. It is one of the very earliest maps we possess. It was made by Langrenus, and dated 1645, and is hardly known in England. A few months ago I wanted to see a copy of it, and not finding one in existence, I suggested, in as disinterested a manner as I could, that our President should write to M. Lœwy, who I knew had recently had it photographed, and that perhaps he might be induced to present a copy to the Society. I am afraid our President was very disinterested, for he asked M. Lœwy if he would be kind enough to send me a copy. M. Lœwy did so, and

in writing to acknowledge his kindness, I said that as the map was so historically valuable I should like to be permitted to present it to the Society in his name. M. Lœwy wrote back asking me to do as I suggested, and at the same time he sent me another copy for myself, which is now shown on the screen. In the original the Moon's diameter is 35 centimetres, and the photograph is reduced to about half the size—it is just over 18 centimetres. It was engraved on copper, as we learn from contemporary sources, by Langrenus himself. There is a long Latin inscription on the map which tells us that Langrenus, in the year 1628, was in Brussels, and that the Infanta Isabella, who I understand was Regent of the Netherlands at the time, was very much interested in his studies, and sent him to Spain, where Philip the Fourth appointed him Astronomer to the Court of Spain. This map was published at King Philip's expense. Langrenus also had the intention of publishing a series of thirty maps, showing the Moon at different phases. None of these maps, I am sorry to say, are in existence now. The only map of the Moon earlier than this is in Brussels, also by Langrenus. The year and date of it are uncertain, but it is very inferior to this. The map in the 'Selenographia' of Hevelius is also dated 1645; the book was published in 1647. The next map was that by Grimaldi, which appears in Riccioli's 'Almagestum'; it bears the date 1651, and was published in 1653. These are the earliest maps of the Moon we have. Round the map of Langrenus is a series of Latin quotations bearing on the Moon.

Mr. Knobel. I think the maps and photographs shown by Dr. Rambaut are extremely interesting. The first drawing made by Russell, that of the full Moon, seems to me quite remarkable, remembering the optical means at his disposal, and is well worth being carefully preserved and reproduced. There has been so much discussion about Linné that it is rather a resuscitation of an old subject, but the question remains whether there are any substantial grounds for the assertions that were made 30 or 40 years ago about Linné; but these early results which Dr. Rambaut has brought before us are exceedingly interesting, and I hope to have the opportunity and pleasure of seeing them reproduced in our 'Monthly Notices.'

The President. I will ask you to return your thanks to Dr. Rambaut and Mr. Saunder. I may perhaps recall a point which is of somewhat melancholy interest—namely, that it was at the suggestion of the late Dr. Common that Mr. Stone had that map of the Moon photographed.

The Astronomer Royal being called on by the President to make some remarks about Comet c, 1903, and about the paper on observations of Leonids made at the Royal Observatory, photographs of the comet were successively thrown on the screen. The first photograph shown was taken on July 24 with the 30-inch mirror made by Dr. Common for the Thompson

Equatorial with exposure of 80 minutes. On the same day photographs were taken by Prof. Barnard at the Lick Observatory with a small short-focus lens, which show a remarkable breaking up of the tail, but unfortunately this part of the tail is just outside the field of the Greenwich photograph. This illustrates the advantage of working with instruments of different scale. The next is a photograph taken with the same instrument on August 1 with an exposure of 45 minutes; the sharp definition of the different tails is very remarkable in this photograph, nine being clearly shown. I should explain that the Sun was in the direction opposite to the longest tail, which is interesting in connection with Bredichin's theory of comets' tails. Here is a photograph taken with the rapid rectilinear lens on 1903, July 26, with an exposure of 3 hours, which shows an extension of the tail of about 6° . The next photograph which I show on the screen is a photograph of Perrine's Comet 1902, taken with the Greenwich 30-inch reflector, and I think it will be seen that there is a remarkable similarity in the character of the tails of the two comets, and comparing the photographs taken by Prof. Barnard of Swift's Comet 1902 and Brooks's Comet 1893, I think it will be seen that there is a remarkable similarity between the four. But these comets seem to differ much in appearance from Donati's and other very bright comets, the tails of which appear to have been of a very different character.

As to the observations of the Leonids at Greenwich. On the morning of November 15 no meteors were seen, though the sky was clear; but on the morning of November 16, about 150 Leonids were seen. Between 2 and 4 A.M. they appeared at the rate of about 20 per hour, but about 6 o'clock this rate was much increased. These may be taken as confirmatory of Mr. Denning's observations. It was somewhat unexpected that we should get so many meteors this year after the comparative failure of recent years.

Mr. Whittaker said that Mr. Denning had sent a paper on the Leonid meteor shower of 1903. On the morning of November 15, although the sky was clear, practically no meteors were seen; but on the morning of November 16 a large number of Leonids were observed. The number was 18 an hour for a single observer at 1 o'clock in the morning, 50 at 3 o'clock, and 200 an hour at 5 o'clock. This was not quite so large a frequency-number as was found by the American observers in 1901. The meteors of this shower were fairly alike in the matter of brightness, whereas Perseids and Andromedids are usually distributed over a wide range of magnitude. This year only one or two of the Leonids observed were as bright as Jupiter. Mr. Denning at the end of his paper gave some statistics as to the apparent height and velocity of the meteors seen during the last 16 or 17 years.

Mr. J. C. W. Herschel. Does Mr. Denning mention any

meteors of a non-Leonid character coming from the region of the Leonid radiant? I was only watching a quarter of an hour on the morning of the 16th; and of the 12, I saw two that were clear blue and without train at all. They were something like Roman candles, much swifter than the orange, train-accompanied Leonids, yet radiating from within the Sickie, and it would be interesting to hear if anyone else noticed them.

Mr. Whittaker said that Mr. Denning referred to these in his paper.

Mr. Maures Horner. I think the number of meteors seen is rather exaggerated. I counted 86 between 5.30 and 6.15 A.M. and none of them were remarkable for colour. I do not understand what the last speaker means by saying that some were like Roman candles. I saw one as bright as Sirius which came from the radiant-point, and therefore seemed to appear and then go out again without any change of position. I do not think anything much yellower than Arcturus was seen. I should say that the majority of them were very quick moving, and there were very few that I could see which came from any other radiant. I should certainly say that the position of radiant was decidedly east and north of γ Leonis; and that may still correspond with what Mr. Denning has said, because he gives a large area for the position of the radiant.

Dr. W. J. S. Lockyer. Mr. Denning refers to the radiant-point. From observations made at South Kensington it would seem that there were practically two radiant-points. Perhaps Mr. Denning can corroborate and explain that?

Mr. Whittaker said that Mr. Denning mentioned radiant-points in a region of six degrees square.

Capt. Noble. When Mr. Denning speaks of the velocity of the meteors presumably he means their velocity after they have entered our atmosphere.

The President. I am sure you will all return your thanks to the Astronomer Royal and to Mr. Denning.

Mr. J. C. W. Herschel then read a paper on an examination of the relative number of stars photographed in different parts of the plates forming the Harvard photographic star map. The plates were taken with a portrait-lens of 1 inch aperture and 14 inches focal length, and covered a field of 30° square. He based his conclusions on 12 out of 55 plates, on which he counted the number of stars along diagonals. The gist of the whole paper was in a diagram which he would put on the screen. Photographs could only rank as *maps* if magnitudes were represented with reasonable uniformity all over the field, and the curve in the diagram showed that there *was* very satisfactory uniformity over an area 18 degrees in diameter. This was additional evidence in favour of using a doublet rather than a singlet for charting purposes.

The President. Fellows will remember that Prof. Pickering has sent a copy of these maps to the Society, with a request that

the Fellows might use them freely. Mr. Herschel has made a preliminary step in demonstrating their value. I will ask Mr. Crommelin to read his note about the rotation of Saturn.

Mr. Crommelin said that some ten years ago the late Mr. Marth gave an ephemeris for physical observations of Saturn. There had been little work done on the subject of Saturn's rotation until the present year. Mr. Stanley Williams, in vol. 54 of the 'Monthly Notices,' gave a summary of what had been done up to that time, and also supplied some determinations of his own, derived from about twelve different markings; his values ranged from $10^h 13^m$ to $10^h 15^m$, from which it appeared that the variations in the rate of rotation of the planet were small. Barnard's spot of the present year, however, gave a period of $10^h 38^m$, so that they had a range of 25 minutes between different regions of the planet, five times the range for Jupiter. He hoped now that interest had been awakened in the subject more work would be done in reference to it. He had thought it well to make the ephemeris retrospective as well as prospective, to assist in the reduction of the observations of last summer. The ephemeris contained the longitudes of the central meridians and the times of transit of the zero meridian for two values of the rotation period ($10^h 14^m$ and $10^h 38^m$). The position-angle of the axis of Saturn might be taken the same as that of the ring which was in the 'Nautical Almanac.' The declinations of Earth and Sun above the ring-plane were also given there.

Mr. Dyson then read a paper by *Prof. Hough* (Chicago) on the rotation period of the planet Saturn. Mr. Dyson said that last month a paper by Mr. Stanley Williams on the white spots was read. This paper by Prof. Hough was on the white spots first observed by Prof. Barnard on June 17.

Dr. H. N. Russell. It is remarkable what an enormous difference in the linear velocity corresponds to this observed difference between the rotation of different zones of the planet. The relative velocity must be about 1600 miles an hour to account for these differences in the rotation period.

The President. I am sure we are under a great obligation to Mr. Crommelin for making these ephemerides of different kinds, and it is an instance of the thorough way in which he does it that he has extended this ephemeris of Saturn backwards entirely on his own initiative, and thereby enabled observers to compare their past observations with the ephemeris.

The President. I have a small paper on graphical methods for finding the time of sunset at any place. The usual method of determining sunset takes account of latitude and longitude in separate calculations or tables, but if we refer to the origin of the phenomenon we shall find there is a much simpler way of considering it. The line of shadow advances over the globe at a uniform rate. If we make a map on which the meridians are parallel equidistant lines, this shadow-line will slide uniformly

across the map. By choosing an arbitrary and variable scale of latitudes, the shadow-line will be sensibly straight for all dates, and we then only need a time-scale to complete the diagrammatic solution. The time-scale may give either (a) the absolute time of sunset or (b) the *difference* of local sunset from that at a standard place such as Greenwich. The advantage of (b) is that we get a more open scale, for the time of sunset at Greenwich varies 4 hours according to the time of year; but the *difference* of sunset time from that at Greenwich only varies about 40 minutes throughout England. We can represent 40 minutes on a much more open scale than 4 hours. The actual method which I had arrived at a year ago for showing the time of sunset diagrammatically is sketched on this map, which has been on view in the library and will be handed round. When at Southport last September I found that Mr. Benson had been working at the problem and had arrived at a solution which is essentially identical with mine, and which is illustrated in these very convenient maps, which will also be handed round.

Mr. Benson. I do not think I have much of consequence to add to what has been said. Mr. Clark and I began to interest ourselves in this matter in 1898, and we brought out a chart monthly, but we found that dealing with it monthly for twelve months made a bulky thing of it, so we thought out a method which would make one chart do for a year. The error for leap-year only amounts to a few minutes, and the accuracy to which the time can be read is something about that limit.

Mr. Crommelin. The orthographic chart of the world which was brought out in the 'Geographical Journal' by Major Grant about eight years ago can be used for finding the apparent time of sunset with very little trouble. It is simply a circle on which the meridians and parallels are drawn as seen from an infinite distance. One has only to draw a line through the centre inclined to the vertical, at the angle of the Sun's declination, intercepting the various parallels of latitude at points whose distances from the edges of the map give the local apparent times of sunrise and sunset. The diagram is very useful for places for which local tables are not available.

The President. The next paper is by *Mr. Cowell* on the Theory of the Moon, in continuation of the paper on that subject read by him at the last meeting.

Mr. Dyson read the paper by *Mr. Cowell*. He said that as Mr. Cowell explained last time he was engaged on a comparison of observations with the tabular positions of the Moon from 1755 to the present time. In this paper he treated the observations from 1847 to 1901, as material to determine the coefficients of the variation and the parallactic inequality. From the parallactic inequality he derived $8''.77$ as the value of the solar parallax. The coefficient of the variation agreed with Hansen's empirical

correction of $0''.3$ to allow for an assumed difference between the Moon's centre of figure and centre of gravity.

Mr. Saunder in reply to a question by the President said : I am afraid I cannot at the moment recall the particulars of Hansen's paper which appeared in the Memoirs of the Society, but I remember one interesting suggestion which Hansen made in it and which has attracted the attention of novelists. He said that as the centre of inertia of the Moon was further from us than its centre of figure, it was quite possible that the lunar atmosphere and any water there might be on the surface had all been drawn round to the other side, and that consequently the other side might be habitable and inhabited.

The Astronomer Royal. I think, apart from the special interest in regard to the solar parallax, this is of great interest as showing the very skilful method of computation which Mr. Cowell has devised which enables him to deal with this great mass of work, because he has done in the course of a month what in the ordinary course would have taken many months to do—namely, applying Newcomb's and other corrections to Hansen's tables and reducing them to one harmonious system. In determining the parallactic inequality there is of course the difficulty that the value of the Moon's semi-diameter is mixed up with it and with the variation ; and as, naturally, there have been changes in the staff of observers at Greenwich, and as for the first few years a different instrument was used, there may be differences in the semi-diameter as observed by the different observers ; but I do not think that would affect the result, for Mr. Cowell has shown that these changes which may have taken place may be considered to compensate for one another in the long run, and that they will be fairly equally distributed, so that no sensible error will enter into the result he has obtained. But that is the difficulty of the problem, and I think Mr. Cowell in dealing with this mass of figures has fairly overcome it.

Mr. Crommelin said Mr. Cowell reduced the semi-diameter by $0''.6$ and made it only $0''.8$ greater than Struve's semi-diameter found from occultations, and that lessened considerably the amount of refraction they could allow at the Moon's limb in occultations. The difference was partially due to irradiation, so that if Mr. Cowell's semi-diameter was correct the refraction could only be a fraction of a second of arc ; it was formerly thought that it might be nearly two seconds of arc. He thought Mr. Thackeray deduced a few years ago a value of the diameter from observations of the full Moon when the first and second limb could both be observed. This practically satisfied Hansen's diameter, but in this case Mr. Cowell had used all the observations, not only those of full Moon ; so it looked as if the diameter got from the full Moon only was slightly different from the average Moon. He thought Prof. Newcomb, while not believing in

Hansen's explanation of eccentricity of the Moon's centre of gravity, said that irradiation produced a spurious term which more or less satisfied Hansen's empirical variation coefficient.

The President. I am sure we shall concur in what the Astronomer Royal has said about the importance of this work and the admiration we feel for the rapidity with which it is being done by Mr. Cowell.

Mr. H. C. Plummer then read a paper on Oscillating Satellites, in continuation of a paper published in the last two numbers of 'Monthly Notices.'

Mr. Whittaker. If I say anything it will be by way of justification of the general class of researches to which this investigation belongs: for one cannot but notice sometimes, among those who are not specially attracted by the mathematical side of astronomy, some attitude of doubt as to whether the results to be obtained in this region quite justify the labour which they entail. On this point I would say that, since mathematicians now recognize the impossibility of integrating the differential equations of most dynamical problems in terms of known functions, the most hopeful line of advance seems to be an investigation of the general properties of orbits—the discovery of periodic orbits, the settlement of the question as to which orbits have infinite branches and which are contained wholly within a limited region, and so forth—in fact, what might be called a natural history of orbits. The electron-theory of electrodynamics leads us to hope that a theory of orbits may be able to explain many phenomena of radiation, and we welcome Mr. Plummer's paper as a contribution to a subject which may prove to be of paramount importance in the near future.

The President. I shall not venture to add anything to what Mr. Whittaker has so eloquently said about the value of this kind of work. I am interested in learning that he himself is working in this direction, and we may possibly have another paper from him at no distant date. I think Mr. Maunder has received a rather interesting letter from Mr. Percival Lowell which the Fellows will be glad to hear.

Mr. Maunder. It will be remembered that Mr. Evans and myself communicated a paper to the Society last June on the actuality of the "Canals" of Mars. Shortly after this Mr. Lowell sent me a copy of the 'Bulletin' of the Lowell Observatory in which he described some experiments on the limit of vision for a wire in the open air. It happened that my wife and I had made a series of experiments in the same direction and also on the limits of vision for dots and lines drawn on a surface, and that we had communicated a paper on that subject to the June meeting of the British Astronomical Association. I therefore sent a copy of both papers to Mr. Lowell, and drew his attention to the difference which we had found between the case of a line or wire

in space, and seen against the sky, and that of a line drawn on a surface, the former being visible twice as far as the latter. Mr. Lowell has replied to this, and his letter reached me just as I was leaving home to come to this Meeting, and Mr. Lowell seems to desire that it should be read to you:—

Pray accept my thanks for the pamphlets you have sent me on the visibility of lines and “canals.” As you were of opinion that lines on a disk would not be so evident as lines against the sky, I had experiments made to that end—a transcript of which I enclose to you and which I will beg you to return at your convenience. Such difficulty as was found was only due to the swinging of the disk in and out of the sunlight; otherwise the results are very fairly in accord with those got for a wire, as you will see by comparing the distances with those in my published pamphlet. As to a diffraction or interference effect, as it is better called, I do not see that this would differ theoretically in the one case from the other, as the background does not affect it.

As to your experiments on dots and shadings giving the appearance of lines, such experimental physiology is very good and useful, but beside the point astronomically for two reasons:—First, because that a may produce the effect of b furnishes no proof or even presumption that a is b , since b itself would produce the same effect. Secondly, the observations at Flagstaff are of a much greater definiteness than is supposed in England. You might, for instance, as well say that a telegraph line as seen from the street was merely an optical effect of dots and shadings. This is a matter of astronomical experiment and not of psychology discussion—the eye may be deceived below a certain limit but not above it.

I may take this occasion to say that the markings on the planets Venus and Mercury are not, as supposed in England, “canals” at all—they bear no resemblance whatever to those on Mars, the difficulty of properly depicting them alone accounting for any seeming resemblance. I may further add that the markings on Venus are certainly real.

Permit me in conclusion to point out to you—which I think you will see the force of—that the only evidence germane to the matter is to be got from astronomical observations directed to that end. If England would only send out an expedition to steady air with acute-sighted not sensitive observers (see my ‘Bulletin’ for the distinction), it would soon convince itself of these realities.

You are welcome to read this letter before the Royal Astronomical Society—if you care to—as I see there is much misapprehension on the subject.

The President. It is very interesting to know that Mr. Lowell stands to his guns in spite of the paper which Mr. Maunder read lately, and possibly during one of his visits to England we may hear him on this interesting topic.

The following papers were announced and partly read:—

G. W. Hough. “The Rotation Period of the Planet Saturn.”

W. F. Denning. “The Shower of Leonids in 1903.”

A. A. Rambaut. Two drawings of the Mare Serenitatis by John Russell, R.A., affording some hitherto unpublished evidence as to the appearance of Linné in the year 1788.

H. H. Turner. “On Graphical Methods of finding local or

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Greenwich time of Sunset at different places within a given region."

H. C. Plummer. "On Oscillating Satellites." Second paper.

J. C. W. Herschel. "An Examination of the Relative Star Density in different parts of the plates forming the Harvard Photographic Star Map."

Royal Observatory, Greenwich. "Observations of the Leonid Meteors of 1903."

P. H. Cowell. "On the Semi-diameter, Parallaxic Inequality, and Variation of the Moon from Greenwich Meridian Observations 1847.0 to 1901.5."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Saturn 1903-4."

Royal Observatory, Greenwich. Note on Comet (Borrelly) c 1903.

The following gentlemen were elected Fellows of the Society:—

Commander Philip Dumas, R.N., F.R.G.S., Royal Naval Barracks, Chatham.

Henry Eichbaum, 3 Devonshire Terrace, Ventnor, I. of Wight.

Frank Flowers, Map Office, Ginsberg Chambers, Johannesburg, Transvaal, South Africa.

Louis George Macrory, M.D., M.B., B.C., B.A., Clifton House, Bridge Road, Battersea, S.W.

The following Candidates were proposed for election as Fellows of the Society:—

J. D. Bharda, Joint Principal, New High School, Bombay, India (proposed by A. Fowler).

Tyson Crawford, Optician (Messrs. Dollond & Co.), 35 Ludgate Hill, E.C. (proposed by W. H. Maw).

E. Vincent Howard, Hereward Cottage, Westgate-on-Sea (proposed by A. C. D. Crommelin).

Eber Jachin Sharpe, Ex-Master, Board of Trade, Nautical Surveyor, Newport, Monmouthshire (proposed by Mr. J. O'Sullivan).

E. Spiegel, Banker, 120 Bishopsgate Street Within, E.C. (proposed by R. T. A. Innes).

Philip Edward Vizard, Chief Clerk of the Summons and Order Department, Royal Courts of Justice, 3 Pilgrim Lane, Hampstead, N.W. (proposed by the Hon. Justice Bruce).

MEETING OF THE BRITISH ASTRONOMICAL
ASSOCIATION.

President: S. A. SAUNDER, M.A., F.R.A.S.

Secretaries: A. C. D. CROMMELIN, B.A., F.R.A.S., and
J. G. PETRIE, F.R.A.S.

THE first ordinary Meeting of the fourteenth Session of the British Astronomical Association was held at Sion College on Wednesday, November 25.

The names of six Candidates for Membership were read and passed for suspension, and the election by the Council of 12 new Members was unanimously confirmed.

Mr. A. C. D. Crommelin said that *Mr. Packer* had written to the Council suggesting a new Section for Nebulæ. The Council had decided that the number of Sections was already very large, and that the most effective way of studying nebulæ was certainly by photography. They thought the best way would be to tack this on as an appendage to the Photographic Section and that members who had not themselves suitable apparatus for taking photographs might study the nebular photographs taken by others, in particular *Dr. Isaac Roberts's* splendid series.

The President said there was a great deal of work to be done on spiral nebulæ. A very well-known mathematical astronomer was, he believed, now studying the question, and they might hope, before many years were over, to hear the results of his work.

A paper by the *Rev. A. L. Cortie, S.J.*, on "Sun-spots and Magnetic Storms" (illustrated by the lantern), was read. The paper described in detail the two great spot groups of October last and the great magnetic storm of the 31st of that month. The big spot appeared near the E. limb on October 5, in latitude -20° , longitude 205° , and was surrounded by a thick patch of brilliant faculæ; it reached its maximum area of about 2300 units on October 10. On October 26 a small group of scattered spots in extensive faculæ was drawn near the E. limb in latitude -20° and longitude 296° , and on the same date a small spot in a bright patch of faculæ was also drawn on the E. limb in position latitude $+16^{\circ}5$ and longitude $232^{\circ}5$ which rapidly increased in size. On October 31 these groups with a combined area of 1361 units were very near the central meridian. It was on this date that the great magnetic storm occurred, the severest ever recorded by the photo-magnetographs at Stonyhurst, which have been in constant operation since 1867. *Mr. Evershed* had written to say that he had been fortunately able to photograph the spot in K light on October 31 and also on November 2nd, and that it was remarkable for very extensive and brilliant reversals of the hydrogen lines, which tended to confirm the opinion that it was only the more active eruptive spots which had so marked an effect on terrestrial magnetism.

The paper concluded with the remark that the particular efficiency of spectroscopically active spots, originally raised by Prof. Tacchini, and again mooted by Mr. Evershed, had been fully discussed by Father Sidgreaves in his paper on the "Connection between Solar Spots and Earth Magnetic Storms."

Mr. E. Walter Maunder said that Father Cortie's admirable paper did not leave him much to say in respect of the general details of these recent great sun-spot appearances. He had several photographs to show which he thought would speak for themselves. The first of these were taken on October 12th, and showed the first of the two great southern groups as seen just after it had passed the central meridian. This was the day of the first magnetic disturbance, which was much less violent than the great one on October 31st. The changes in the group were very rapid, even while observation was being made. Photographs taken 10 minutes apart showed a distinct alteration. This was illustrated by a series of slides including a diagram showing the photographic register of the declination magnet at Greenwich. The next photograph shown was of the Sun on October 31st, corresponding to the last picture in Father Cortie's series of slides. The first spot of the principal group had just reached the central meridian at 10 o'clock on the morning of October 31st. At 6 o'clock that morning the magnetic storm began. Diagrams giving declination and horizontal force registers for that day showed that in both cases the traces went right off the sheets. Mr. Maunder concluded by showing a diagram giving the sun-spot curve from the Zurich observations and the rainfall curve for London from the year 1842 reduced to the same scale. He did not think it would be possible to get two curves much more unlike each other than were these two.

Mr. G. J. Newbegin had three slides thrown on the screen, being photographs taken on November 3, 4, and 5, showing rapid changes in the spot formation.

A paper by Mr. A. J. S. Adams, entitled "Remarks relative to Sun-spots and Terrestrial Magnetic Storms," was next read.

Capt. W. Noble passed round some drawings of recent sun-spots by Mr. Jenkinson, a member of the Association, which he thought were singularly beautiful. He wished to congratulate Mr. Maunder upon the diagram of rainfall and sun-spots.

Mr. G. F. Chambers said he had a very good view of the large group on several occasions, and upon measuring it he found that the longest dimension was more than four minutes of arc, or more than 108,000 miles. What struck him as regarded the great group was that the proportion of penumbra was very much greater than usual as compared with the proportion of umbra.

A Member asked if there were any auroræ observed on the night of Oct. 12-13. In the current number of the *Journal* there was a description of an aurora on that night, and he wished to ask whether it had been confirmed by anyone else.

Mr. Astbury said that five beams appeared to radiate from a common centre, and he described their rotating "like the spokes of a majestic wheel."

Mr. G. M. Seabroke said he took it that the motions of the magnet were indications of changes in the magnetism of the Earth. Would *Mr. Maunder* kindly express the motions of the magnet in terms of the Earth's magnetism?

Mr. Maunder in reply said the declination magnet under ordinary circumstances at Greenwich pointed about 16 degrees west of true north and had a small daily range to the west in the afternoon and back again during the night. The horizontal- and vertical-force magnets were two magnets to measure the intensity of the magnetic force, the force being measured in two components. The greatest motion of the declination magnet was a little over two degrees, the usual daily swing was about seven minutes.

In reply to *Miss C. O. Stevens*, *Mr. Maunder* said a great storm would undoubtedly be registered all the world over and the first preliminary twitch would take place at the same instant of actual time all the world over.

Mr. A. C. D. Crommelin said a rotatory movement of the auroral beams occasionally occurred. He remembered, in the great aurora of 1882, seeing the beams slowly sweep across the sky.

Mr. C. Thwaites said he observed the aurora of 1882 and could corroborate what *Mr. Crommelin* had said.

The President said that he would be a bold man who asserted that any form of aurora was impossible. The finest aurora he had ever seen—it was in Ireland—gave him the impression of a great umbrella opening and shutting overhead. Huge rays seemed to open out over the sky, and then draw in and afterwards shoot out again.

Mr. Maunder showed some photographs of *Borrelly's Comet*, and stated that the report of the Comet Section for the past Session was now in type. They had had several comets under observation during the Session, the last and most interesting, because the brightest, was *Borrelly's Comet*. Four photographs of the comet, taken at Greenwich, were thrown on the screen. All the photographs, when examined closely, showed the tail as if divided into some four or five distinct branches. One of the comets of the year was discovered by a member of the Association, *Mr. John Grigg*. Another member, *Mr. John Tebbutt*, followed its movements and determined its position, whilst a third member, *Mr. Merfield*, computed the elements of the comet from the observations which *Mr. Tebbutt* had supplied, so that the Association could regard that comet as entirely their own.

Mr. Crommelin said that it was curious to note the great similarity between the cometary tails of last year and those of this year. The comets that appeared a little more than a year ago also showed a fan-shaped tail. It was not at all of the ordinary type.

Mr. Tyson Crawford (of the firm of Dollond & Co.) explained the apparatus which he had applied to two telescopes which were on view in the hall. In the first case he had adapted a drawing-board, with screen for sketching, using it with a star diagonal so as to get a projection, say, of the Sun and its spots in a convenient position for sketching. The other apparatus consisted of a new eyepiece for the star-finder. He had a small ring engraved upon the field-lens of the eyepiece, and the form of the eyepiece was one that gave a very large field, some three times the size of that given by the ordinary finder.

In reply to *Mr. Holmes*, *Mr. Crawford* said that the engraved ring was visible with a star of ordinary brilliancy.

The President said he regarded the principle as the correct one. In his own finder he had four wires so arranged as to give very much the same advantage.

Mr. W. H. Maw said that the late *Rev. W. B. Dawes* maintained that the proper arrangement of the cross-wires of a finder was to place them at an angle of 45° with the equatorial direction.

Mr. W. T. Lynn read a paper on "The Floor of the Stellar System."

An Interim Report of the Meteoric Section, dealing with the Leonid shower of the present year, was next read.

Mr. Crommelin said the display must have been of a very striking character. It was noted in quite a casual way by one of the observers at Greenwich, who was intent on getting observations of the Moon. He noted several of them as being as bright as Venus.

Mr. Crommelin thought it was very satisfactory that a determination of velocity had been made, as mentioned in the report, because it entirely disposed of a theory advanced in a paper in the November number of *Knowledge*, which suggested an orbit round the Earth inside the orbit of the Moon. The velocity recorded entirely disposed of that theory.

Mr. Holmes asked how they could possibly tell anything about the velocity in space by ascertaining the velocity of a meteor during its visible path.

The President believed that it was generally admitted that in the case of the November Leonids, *Prof. Adams* had shown that no other orbit than that which was generally attributed to them was admissible.

A communication from *Mr. E. M. Antoniadi*, on "The Instrumentality of Physiological Phenomena in apparently duplicating the Dusky Areas of Mars," was read, in which he described the experiments that led him to formulate the theory published in the pages of the *English Mechanic*.

Capt. Noble said that *Mr. Antoniadi* was doing for areographical wonders very much what *Mr. Maunder* had been doing for the rain and the sun-spots.

The Meeting stood adjourned to the 30th December.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday evening, December 16, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, President, in the Chair.

Mr. W. Marriott gave some account of the Meteorological Work of the late *Mr. James Glaisher, F.R.S.*, who was the founder of the Society in 1850, and who died on February 7th last, at the age of 93 years. *Mr. Glaisher* was appointed Superintendent of the Magnetic and Meteorological Department of the Royal Observatory, Greenwich, on its foundation in 1840. He soon became interested in and conversant with all kinds of meteorological investigation, and through his instrumentality numerous meteorological stations were equipped in various parts of the country. He furnished quarterly the results from these stations to the Registrar General from 1847 up to March 1902. He prepared various Tables of Corrections for the use of these observers, the principal of which was his *Hygrometrical Tables*, which has passed through 9 editions. He wrote numerous papers on meteorological subjects, a bibliographical list of which *Mr. Marriott* appended to his Address. He was a Juror of the Great Exhibition of 1851, and as such he caused a great stimulus to be given to the manufacture of reliable meteorological instruments. *Mr. Glaisher* was most known to the public for the 28 balloon ascents which he made for scientific purposes in 1862-69 on behalf of the British Association Committee. The highest ascent was that from Wolverhampton on September 5th, 1862, when the height of about 7 miles from the earth was reached. *Mr. Glaisher* was rendered insensible while *Mr. Coxwell's* hands were frozen, and he was only able to open the valve of the balloon by tugging at the rope with his teeth. *Mr. Marriott* illustrated his address with numerous interesting lantern slides, and also exhibited the instruments &c. used by *Mr. Glaisher* in his famous balloon ascents, which by the courtesy of his son, *Dr. J. W. L. Glaisher, F.R.S.*, had come into the possession of the Royal Meteorological Society.

A paper by *Mr. J. R. Sutton* on "Certain Relationships between the Diurnal Curves of Barometric Pressure and Vapour Tension at Kenilworth (Kimberly), South Africa," in the absence of the author, was read by the Secretary.

The Planet Jupiter.

BETWEEN May 26 and November 19 of the present year Jupiter was observed here on 84 nights, and 1200 transits taken of the various markings. In the summer months some particularly fine

views were obtained in the morning hours and a large amount of detail was observed, including some irregular markings in the polar regions. But in the months September, October, and November scarcely a single night afforded a really good view, and many of the more delicate features noticed in earlier months were lost.

It is hoped to continue the observations until the close of January next, when the planet will have arrived at a point too near the Sun for further effective work. The results will then be compared and properly reduced.

During the past six months the great red spot has shown a slight retardation of speed, the mean rotation-period having been $9^h 55^m 41^s.7$ as compared with $9^h 55^m 39^s.0$ in 1902.

The large south tropical spot, which has been visible since the spring of 1901, continues well marked, conforming with a motion of $9^h 55^m 19^s$. It extends over 48° of longitude, and its centre will be in conjunction with the centre of the red spot in the first half of June 1904.

A rapidly-moving dark spot in the S. S. temp. region has exhibited a rotation-period of $9^h 55^m 6^s$.

Three of the most conspicuous N. tropical spots (situated on the red, narrow belt N. of the N. equatorial belt) have shown different rates, as follows :—

①		$9^h 55^m 35^s.8$
②		$9 \ 55 \ 31^s.5$
③		$9 \ 55 \ 26^s.6$

But even greater differences have been found in regard to the N. temp. and N. N. temp. spots (of which considerable numbers, both light and dark, have been presented). Six of the best-observed dark spots have moved very slowly, the mean rotation-period having been

$$9^h 55^m 57^s,$$

while six other similar objects have shown periods nearly agreeing with the rate of System II. of the ephemeris, the mean being

$$9^h 55^m 40^s.$$

Great disturbances have affected the equatorial region of the planet, but the motion has been nearly uniform with that of System I., as in the few previous years.

Bishopston, Bristol,
1903, Nov. 22.

W. F. DENNING.

*Note on an early Astronomical Observation recorded in the
Book of Joshua.*

So far as I am aware no astronomer, except the late Savilian Professor of Astronomy, the Rev. C. Pritchard, has ever attempted to treat the words ascribed to Joshua in the tenth chapter and twelfth verse of the book of the Old Testament which bears his name, as in effect an astronomical observation. Yet the words

Sun, stand thou still upon Gibeon ;
And thou, Moon, in the valley of Ajalon,

certainly imply that to the speaker the Sun and the Moon appeared in certain definite directions at the moment of speaking. If we could assure ourselves of the precise station of the speaker they would become rough determinations of the azimuths of the two heavenly bodies mentioned, and would lead at once to inferences as to the time of the day, and the part of the month, possibly also as to the part of the year, when the incident took place. Unfortunately we are not told where Joshua was standing. It has generally been assumed that he was at Upper Beth-horon, from whence, as Prof. Pritchard states in his book 'Nature and Revelation,' the valley of Ajalon bore S. 66° W. and Gibeon S. 60° E.; corresponding, that is, to azimuths from the north point of 114° W. and 120° E. respectively. Given these bearings, it is quite clear that the event could not have happened near sunset, as the late R. A. Proctor suggested, for the Sun must have been in the south-east quadrant, and nearer the east point than the south; nor can the Moon have been "crescent," as Dean Stanley describes it, since she was west of the Sun and therefore past the full.

Prof. Pritchard calculates that if the Sun bore S. 60° E. the time of day would be 9 o'clock A.M. This of course assumes that the Sun was on the equator; in other words, that it was now the equinox; necessarily the autumnal equinox, since the Israelites entered Canaan about the time of the spring equinox. Prof. Pritchard did not, however, go on to compute the altitude of either Sun or Moon. The former, under the assumed conditions, would have had an altitude of about 39° ; the latter, from the difference in the azimuth of the two bodies (126°), must have been very near the most northerly part of the ecliptic, and not far from her "last quarter." Her altitude therefore must have been about 60° . But by the time he had reached Upper Beth-horon, Joshua must have left Gibeon for at least an hour and a half, and have lost sight of it for an hour; for the road between the two places is six miles long, and a rise in the ground hides Gibeon after the first two miles have been traversed. Dean Stanley expressly notes that Gibeon is out of sight from Beth-horon, and the 'Survey of Western Palestine' makes it quite clear that his statement is correct. But no man with the Sun 39° high ever thought or spoke of it as being "upon" some distant town, and most certainly

not as "upon" a town that now was out of sight, and had been so for an hour at least. Nor could the Moon when 60° high be associated with any distant place, as seeming to stand over it. The ordinary man would think of Sun or Moon when 60° high as overhead to himself, and not to places at a distance. Indeed any such association between an object on the Earth and a celestial body is only natural when the latter is quite low down, say 10° high or under. The fancy is a strained one if the celestial object is 15° high, and becomes quite inadmissible if it is more than 20° . From this point of view the words of Joshua become not only an observation of the azimuths of Sun and Moon, but, though very roughly, of their altitudes as well.

Failing any direct statement as to the place or station from which the observation was made, some light may be gathered as to the time of the year. The only positive dates given in the history are "tenth day of the first month" for the passage of the Jordan, and "the fourteenth day" of the same month for the feast of the Passover. As the feast lasted one week, and the siege of Jericho another, the equinox must have been past a full month and more before the battle of Beth-horon took place. But a month after the spring equinox the Sun had attained a declination of 12° N., and it did not again decline below this declination until eighteen weeks later on. Seeing how exceedingly close together all the places mentioned in the narrative are, it seems reasonable to conclude that the battle took place at some time within this period, which is more than one-third of the whole year in length.

The following table (p. 59) gives the altitude and azimuth of the Sun in the latitude of Gibeon, $31^\circ 51' N.$, at every hour from sunrise to noon and from noon to sunset, for the declinations $12^\circ N.$, $18^\circ N.$, $20^\circ N.$, and $23^\circ 52' N.$; the last named being approximately the declination of the Sun at the summer solstice at the time of the conquest of Canaan. The azimuths are reckoned from the north point of the horizon round to the south: through the east point before noon; through the west point after noon. The altitudes have not been corrected for refraction.

Assuming, as generally has been done, that Joshua saw Gibeon in the distance in one direction, with the Sun low down in the sky—less than 20° high—immediately above it, and that he saw the valley of Ajalon in another direction, with the Moon low down in the sky immediately above it, can we find where he must have been standing to have had these appearances presented to him?

If we take Gibeon as centre and describe a circle round it, we may divide that circle into six equal portions, three to the east and three to the west of the meridian. Clearly Joshua was not anywhere in the two southern sextants, for the Sun in that latitude can never have an azimuth as reckoned from the north point of less than $61^\circ 5'$. It follows, therefore, that to an observer placed

TABLE I.—Altitude and Azimuth of the Sun at Gibeon.

A.M.	12°		18°		20°		23° 52'		P.M.
	Alt.	Az.	Alt.	Az.	Alt.	Az.	Alt.	Az.	
h m	o	o	o	o	o	o	o	o	h m
4 56							0°0	61°5	7 4
5 0							0°6	62°0	7 0
5 8						66°8	2°2	63°0	6 52
5 13			0°0	68°7	1°0	67°4	3°1	63°6	6 47
5 30	0°0	75°8	3°3	70°9	4°4	69°1	6°4	65°8	6 30
6 0	6°3	79°8	9°4	74°6	10°4	72°8	12°3	69°4	6 0
7 0	19°0	87°4	21°9	81°8	22°8	79°9	24°5	76°1	5 0
8 0	31°7	95°4	34°6	89°2	35°4	86°9	37°0	82°6	4 0
9 0	44°2	105°1	47°3	97°8	48°1	95°2	49°7	89°7	3 0
10 0	56°0	118°9	59°6	110°1	60°7	106°4	62°4	69°3	2 0
11 0	65°8	141°8	70°6	132°1	72°1	127°8	74°5	117°4	1 0
Noon.	70°2	180°0	76°2	180°0	78°2	180°0	82°0	180°0	Noon.

in the two southern sextants the Sun could never, at any time of the day or on any day of the year, appear to be over Gibeon.

The two northern sextants are likewise excluded. During the eighteen weeks in question, the Sun, when it has an azimuth from the north point of 120°, has an altitude of from 56° to 75°. But the greatest altitude which the Sun ever attains in the latitude of London, even at noonday in midsummer, is only 62°. No one either would or could dream of associating so high a Sun with any distant object; it would give no sense of connection with any place within the spectator's view.

There remain therefore only the two last sextants: the one due east and the other due west. The eastern one is excluded, for from this district Gibeon and the valley of Ajalon would be nearly in the same direction. This would mean that the Moon was very near to the Sun in the sky, and was consequently near conjunction. It is perfectly certain that she could not be so seen in a sub-tropical country like Southern Palestine.

We are therefore shut up to the western sextant. From this point of view, the Sun being in the east, would have but recently risen; whilst the Moon would be in the west, near her setting. She would be therefore nearly in opposition to the Sun and near the full. But when the Sun has a high northern declination, the Moon when full must have a considerable declination south. She could not therefore have appeared to be north of the west point, but must have borne south of it. But the valley of Ajalon bears from Gibeon about 17° north of west. Under these circumstances, then, the Moon could not have been seen over the valley of Ajalon. Joshua could not have been standing anywhere in the

western sextant, and we have already seen that he could have been nowhere in the other five.

This result is striking, and shows that there must be some error in the notion generally entertained as to the apparent positions in the sky of the two bodies. Is there any other position which Sun or Moon might hold which would lead a man to think of either as being distinctly and definitely "upon" a certain place? There is one—the most natural of all to give this impression. The Sun might be in the zenith. Then the observer would not only naturally but inevitably speak of it as being "over" or "upon" himself and the place where he happened to be. Adopting this interpretation, Joshua must have been actually at Gibeon when he spoke, and the time must have been nearly noon.

The expression "Thou, Moon, in the valley of Ajalon," has now a very definite signification. The valley of Ajalon bearing 17° N. of W. from Gibeon, the azimuth of the Moon must have been 73° from the north point. The following table gives the hour-angle and declination of an object having this azimuth in latitude $31^{\circ} 51'$ N. for every 3° of altitude from the horizon up to 30° . There is no use in carrying the table further, since it extends to practically the greatest declination which the Moon can have.

TABLE II.—Hour-angle and Declination of a Celestial Object bearing 17° N. of W. in N. Latitude $31^{\circ} 51'$ N.

Altitude.	Hour-angle.	Declination.	Minimum Longitude for the Moon.
0°	0°	0°	0°
3	$99^{\circ} 2$	$14^{\circ} 4$ N.	24
6	$96^{\circ} 6$	$16^{\circ} 0$	$28\frac{1}{2}$
9	$93^{\circ} 9$	$17^{\circ} 6$	33
12	$91^{\circ} 2$	$19^{\circ} 1$	$37\frac{1}{2}$
15	$88^{\circ} 4$	$20^{\circ} 6$	42
18	$85^{\circ} 6$	$22^{\circ} 1$	47
21	$82^{\circ} 8$	$23^{\circ} 5$	52
24	$79^{\circ} 8$	$24^{\circ} 9$	57
27	$76^{\circ} 8$	$26^{\circ} 2$	63
30	$73^{\circ} 8$	$27^{\circ} 4$	71
	$70^{\circ} 6$	$28^{\circ} 6$	80

The table shows that if the Moon were actually on the horizon at the moment of the observation, it must have had a declination of more than 14° N. We have therefore to find a declination for it between this and 29° N., the latter being the greatest which it can attain.

The hour-angle may be anything between 70° and 100° , so far as the table is concerned. Since at noonday in summer the ecliptic is much higher in the heavens than the equator, and hence

is nearer to the pole of the equator than to its own pole, the difference in longitude between the Sun and the Moon must be less than the difference of hour-angle. The precise amount of this difference will depend upon the longitude assumed for the Sun, but it will be between 4° and 5° . We may take it at the lower value.

Assuming the Sun precisely on the meridian, the longitude of the Moon must have been between 67° and 95° less than that of the Sun. A difference of 90° would indicate that the Moon was at her third quarter, i. e. half full. It is not in the least likely, if her phase were appreciably smaller than the half, that she would be visible at noonday, or, if visible, sufficiently conspicuous to catch the eye. It is not possible to draw an absolutely hard-and-fast line, but assuming that the Moon had not passed her third quarter, and that the Sun was exactly on the meridian, her declination must have lain between $14\frac{1}{2}^{\circ}$ and $17\frac{1}{2}^{\circ}$, and her altitude cannot have exceeded 6° .

If we suppose that the Sun had not yet quite reached the meridian, it allows a wider range for the position of the Moon, but at the same time it tends to throw the date later on in the year. But to put the hour earlier in the day, to put the day later in the year, both tend to bring the Sun lower down in the sky, and so render less appropriate, less natural, the exclamation "Sun, stand thou upon Gibeon."

For it is clear that the summer solstice was already past. If the Moon's path were exactly along the ecliptic we could find her position and that of the Sun at once. But her latitude may amount to $5^{\circ}.3$, and with this maximum value north of the ecliptic her longitude can be as low as 24° , and consequently the Sun's longitude might be 114° , and the day of the year July 17 of our present calendar. This is the earliest day of the year on which the observation can have been made.

The Moon cannot have been far from her third quarter. She cannot have been much before it, as, with the Sun on the meridian, the maximum possible difference of longitude is 95° , which would give the time 10 hours before third quarter; and if the time of day be taken earlier, so as to increase the possible difference of longitude between the Sun and Moon, a day earlier in the month would involve changing the time of day by about an hour earlier, and at the same time the day of the year by about a fortnight later. She cannot have been much past her third quarter, for then she would have been too small and faint to be readily seen in the noonday sky. The day of the month, then, may be taken as between the 19th and 22nd of the Jewish reckoning, say the 21st.

But the Moon could not have so high a declination as 14° N. on the 21st day of the month, earlier than the fourth month of the Jewish year.

The event which in the narrative immediately precedes the treaty

which the Hivites of Gibeon made with the Israelites was the reading of the Law on Mt. Ebal, for which the traditional date is the day of Pentecost, the third day of the third month. The Hivites must certainly have tried to carry out their negotiations as quickly as possible, for they had to deceive two parties. They had to make the Israelites believe that they were strangers from an immense distance, whereas they were actually their nearest neighbours, for Beeroth, one of the Hivite cities, was less than four English miles from Ai, which the Israelites had taken. They had also to carry the negotiations through before any suspicion of their defection was entertained by the Amorites; and Gibeon, the Hivite capital, was less than six English miles from Jerusalem, the Amorite capital. Clearly their action must have been prompt. After the treaty was made all parties evidently lost no time. The Amorites attacked Gibeon at once, and Joshua relieved it within 24 hours of learning of its danger.

Now from the third day of the third month to the 21st day of the fourth month is nearly seven weeks, but to the 21st day of the fifth month is eleven weeks. The latter interval seems inadmissibly long. The battle therefore must have taken place on or about the 21st day of the fourth month of the Jewish year.

This fixes the latest possible date in the year as about July 27 of our present calendar, and gives July 22 as a mean date. July 27 involves a longitude for the Sun of 124° , and a maximum difference of longitude between the Sun and Moon of 100° , or a difference of hour-angle of less than seven hours, or 105° . This maximum longitude for the Sun implies several limits for the place of the Moon. Its maximum longitude must have been about 34° , which, from Table II., involves also a maximum possible altitude of 7° , with a latitude of 4° or 5° north, and a minimum hour-angle of 93° . The latter confines the Sun within 12° of hour-angle of the meridian, or three-quarters of an hour of time.

Summarizing the whole enquiry, the words uttered by Joshua, treated as an astronomical observation, involve that he was standing at Gibeon itself; the time of day was noon, or very nearly so, and the Sun within 15° of the zenith; the date was about July 22 of our present calendar, and was the 21st day of the fourth month of the Jewish calendar; the Sun's declination was about 21° N., and the Sun rose almost exactly at 5 A.M., and set almost exactly at 7 P.M.; the Moon was about her third quarter, her latitude being about 5° N., and her altitude under 7° . She had risen about 11 o'clock the previous night, and was now within about half an hour of setting.

The very rough indications given of the apparent places of the Sun and Moon have therefore proved much fuller of information than might have been expected. There is one other deduction from them which it does not seem possible to make. They are not sufficient to fix the year. From the nature of the luni-solar year,

there will always be one or two years in each cycle of nineteen years which will satisfy the conditions of the case, and the date of the Hebrew invasion of Palestine is not known with sufficient certainty to limit the enquiry to any particular cycle.

E. WALTER MAUNDER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Galileo and Marius.

GENTLEMEN,—

I should not have troubled you on this subject again had it not been for the recent appearance of the excellent life of Galileo by Mr. Fahie, in which we read :—" As usual, Galileo's right to the first discovery of Jupiter's satellites was contested, the claimant being Simon Mayer (*sic*), of Anspach, whom we have met before in connection with the Capra plagiarism (p. 45, *ante*). In 1614 he published at Nuremberg his 'Mundus Jovialis,' in which he formulated his claims" (p. 107, note). I have already referred to the fact that Mayr had mentioned his discovery in a pamphlet (now very rare) which appeared in 1612, two years before the publication of the 'Mundus Jovialis.' But that is not the main point, which is (as has been recently shown in 'Galilée et Marius,' by J. A. C. Oudemans and J. Bosscha) that he made no claim to *priority* in the discovery, but clearly states that his dates were in Old or Julian Style, and that he first came to the conviction that the little bodies near Jupiter were satellites of the planet (at first supposed to be fixed stars) on the 29th of December, 1609. Galileo must have overlooked this, for, in his 'Il Saggiatore,' he says that Mayr speaks of his discovery as "faite en 1609, et néglige d'avertir le lecteur que s'étant séparé de notre Église, et n'ayant pas accepté l'émendation Grégorienne, le 7 janvier 1610 de nous autres catholiques coïncide avec le 28 décembre des hérétiques." There was then no occasion to be anxious to disprove the independence of Mayr's discovery, and it was quite unworthy of Galileo to suggest that the former had never seen the satellites at all. Moreover, this fact renders Mr. Fahie's reference to Kepler speaking of "the glorious discovery of the Medicean stars by Galileo" quite irrelevant, as of course the principal credit always rests with the first discoverer *. As to the Capra affair, Prof. Oudemans points out that Galileo quite misstates the matter. So far from Mayr writing the book under Capra's name which claimed the invention of the geometrical and

* Prof. Oudemans points out that reference was made by Galileo to Kepler, but that the answer seems to have been suppressed.

military compass, and hastily quitting Italy in 1607 when the plagiary was exposed, Prof. Oudemans shows that he had left Padua early in 1605, and was passing over the Alps from Italy into Germany in the month of July in that year.

When Mr. Fahie speaks, in p. 46, of Mayr "arrogating to himself the merit of two of Galileo's astronomical discoveries," he means those of Jupiter's satellites and of the solar spots. With regard to the latter, there is no reason to doubt the independence of several observers—Galileo, Mayr, Fabricius, and Harriot—but it is very difficult to determine which had the priority. As Prof. Newcomb remarks (Pop. Astr. p. 250), the mere perception "required neither industry nor skill." But, as I mentioned in the *Observatory* (vol. xiv. p. 129), it was from the observations of the spots by David and John Fabricius that the knowledge of the Sun's axial rotation first resulted. No claim for this was made by or on behalf of Mayr. But it is well known that he was a careful observer, and it is incontrovertible that he was the first to telescopically see and describe the great nebula in Andromeda. Yet it was long after this that Galileo permitted himself to call in question the fact that Mayr had seen the satellites of Jupiter at all!

I trust I shall not be suspected in the above remarks of desiring to diminish in any way the great value of Mr. Fahie's 'Life of Galileo.' I am only sorry that he did not see, before completing it, the pamphlet of Prof. Oudemans, which carries conviction with its careful perusal. In common with many others, I always believed, until I read that pamphlet, that Mayr did claim to be the first discoverer of the satellites. Those who concern themselves much with history and dates between 1582 and 1752 must often have noticed how many mistakes have arisen from forgetting the difference of the styles, and sometimes doubt, with Prof. Newcomb, whether the change was worth making. Mayr died in 1624, the year after Galileo's attack upon him.

Yours faithfully,

W. T. LYNN.

Blackheath, 1903, Nov. 5.

Periodical Comets due in 1904.

GENTLEMEN,—

Faye's Comet failed us last year for the first time since its discovery in 1843, having been very unfavourably placed for visibility at the recent return.

D'Arrest's periodical comet has passed its perihelion, but will be nearest the Earth about the middle of January, when, however, its apparent place will be too near the Sun to afford any hope of its becoming visible.

The comet commonly called Winnecke's, because its orbit was not determined until its rediscovery by Winnecke in 1858 (it was first seen by Pons in 1819), was observed at the returns of

1869, 1875, 1886, 1892, and 1898, passing its perihelion on the last occasion on the 20th of March. It should therefore return again early next year.

The comet discovered by Tempel at Milan on the 3rd of July, 1873, was determined to have a period of about $5\frac{1}{4}$ years in length. Observed again in the autumn of 1878, it escaped perception in 1883 and 1889, being on those occasions unfavourably situated. But on both the last returns, in 1894 and 1899, it was observed, passing its perihelion in the latter of those years on the 28th of July. Another return therefore will be due towards the end of 1904.

Finally we have to expect our old friend Encke. We are all too familiar with its history to need any repetition of this. First seen by Méchain in 1786, its orbit was not determined satisfactorily until its return in 1818-19, when it was first observed by Pons in November, and passed its perihelion in January. Since then it has been seen at every return, the last perihelion passage taking place on the 15th of September, 1901, so that we may confidently expect to see it again in the closing months of 1904.

Yours faithfully,

Blackheath, 1903, Dec. 14.

W. T. LYNN.

The Orbit of β Lyrae.

GENTLEMEN,—

In the short abstract of my paper, "Spectrographic Study of β Lyrae," which I gave at the November meeting of the R. A. S., I alluded to an objection to an elliptic orbit, and said that it was fully stated in the paper. On reconsidering the argument on the press proof sheet, I came to the conclusion that it rested on too slender a foundation, and crossed out the paragraph, not adverting to my remark at the meeting. A word of explanation is therefore necessary. The difficulty arose out of the method followed in the paper for deducing the relative velocity of the stars from the oscillation amplitude of the red-side edge of bright $H\gamma$ with reference to the centre of the dark line. From the tabulated displacements four epochs appear—two of zero-displacement, and two of maximum opposite displacements. The numbers of plates from which these epochs are obtained are very insufficient for localization on the orbit; and of the four epochs, as they appear, two are in favour of the ellipse and two are against it, while all four are impossible in a circular orbit.

I am, Gentlemen,

Yours faithfully,

Stonyhurst College Observatory,
1903, Dec. 14.

WALTER SIDGREAVES.

OBSERVATORIES.

[Continued from vol. xxvi. p. 464.]

POTSDAM (Astrophysical). *H. C. Vogel*.—With the 80-cm. refractor and spectrograph III. 228 star-spectra were obtained, and measures in line of sight of 8 objects. With the 32.5-cm. refractor and spectrograph IV. 144 star-spectra were obtained. Spectroscopic binaries α Persei and ϵ Aurigæ discovered. Prof. Müller investigated the telluric lines in the solar spectrum by Cornu's method. Professors Deichmüller and Kempf continued their work on the Northern Photometric Durchmusterung. Zone $+40^\circ$ to $+60^\circ$ is now complete, the catalogue containing 4108 stars, besides a few in Zone $+20^\circ$ to $+40^\circ$. Nova Persei. Variables. 52 photos of Sun. For the Photographic Chart Dr. Scholtz measured 77 plates and catalogued 16,370 stars. Vol. III. of the Catalogue is now in the hands of the printers.

STOCKHOLM. *K. Böhlin*.—A new working catalogue, consisting of stars from the Radcliffe Catalogue, 1845, and the Carrington Catalogue, has been made out. With the astrographic refractor 89 plates have been exposed for photos of nebulae and for parallaxes. Nova Persei on six plates gave the following magnitudes:—

1902 Jan. 4	8.0	1902 Mar. 9	8.3
Jan. 31	8.3	Dec. 5	10.0
Feb. 2	8.4	Dec. 6	10.0

STRASBURG. *E. Becker*.—The large refractor was used by Dr. Kobold for securing positions of nebulae and for measuring the AZ arc in Perseus; the 18-inch for positions of 117 nebulae. 53 positions of Comet 1902 *b* and 6 of 1902 *d*; Saturn's satellites; diameter of Neptune; lunar coordinates. The meridian observations include a large number of circumpolars, Eros stars, and small planets.

UTRECHT. *A. Nyland*.—Observations of variables, Mira Ceti, δ Cephei, Nova Persei, β Lyrae, η Aquilæ, ζ Geminorum, Meteors, small planets, Gegenschein. Sketches of Jupiter. Observations of Comets 1902 III. and 1902 *d*.

WIEN. (*M. Edler v. Kuffner*) *L. de Ball*.—The meridian zone completed. Observations in Prime Vertical.

ZURICH. *A. Wolfer*.—The chief work is on the Sun, which was examined on 253 days by projected image (25 cm. diameter). The chromosphere and prominences were examined on 65 days. The work is thus shown:—

Observation days.	Rel. No.	Free from spots.
1901 291	2.6	230
1902 276	4.7	198

FLOWER OBSERVATORY (PHILADELPHIA). *C. Doolittle*. (This report is also taken from the 'Vierteljahrsschrift.')—The Flower

Observatory forms a department of the University of Pennsylvania. It is situated a short distance outside the city, on a tract of land of 100 acres, being a portion of the property presented to the University by Reese Wall Flower. Work started in 1896, the original plan being to make observations for latitude variation on the seven-year plan proposed by Küstner. This seven-year period ends 1903 October, and there will be some 1200 determinations of latitude made with the same instrument, star-list, and observer. Incidentally, the following determinations of the constant of aberration have been made:—

1896–1898	a value of	20".580	(20.457)
1898–1899	"	20".540	(20.450)
1900–1901	"	20".561	(20.469)
1901–1902	"	20".513	(20.470)

A comparison of these with the determinations at Columbia University, New York, is interesting. These are given in brackets.

The equatorial telescope of 18-inch aperture is by Brashear. Double stars have been divided with separation 0".2, the theoretical limit being 0".245. The work done with this instrument comprises measures of some 2000 double stars.

NOTES.

MINOR PLANET NOTES.—Herr Riem points out, in *Ast. Nach.* 3916, that the planets (366) Vincentina and (386) Siegena approach one another within some 5 millions of miles on Jan. 21, and invites observations. The following abridged ephemeris may be sufficient for picking them up:—

	(386).				(366).		
	R.A.	Dec.	Mag.		R.A.	Dec.	*Mag.
	h m s	° '			h m s	° '	
Jan. 15	12 9 26	−1 22	} 11.5		12 12 20	−1 7	} 12.9
21	12 10 16	−1 2			12 12 51	−1 24	
27	12 10 24	−0 36			12 12 39	−1 38	

New planets MW, MX (mags. 13.5, 13.2) were discovered by Wolf at Heidelberg on Oct. 27, Nov. 14. The following planets have received permanent numbers: LO 507, LQ 508, LB 509, LT 510, LU 511, LV 512; LP is identical with 406; LS, LW, LX were not sufficiently observed to receive permanent numbers. JO had been numbered 489, but proved to be identical with 470 Kilia. The number 489 has been given to 1902 JM. A. C. D. C.

DISTRIBUTION OF STARS.—A very interesting research has recently been published by Prof. Pickering under this heading, forming Part v. Vol. xlviii. of the Harvard 'Annals,' which may be partly epitomized as follows:—From the hypotheses (1) that stars are equally bright, (2) that they are equally distributed in space, and (3) that there is no absorbing medium, together with the general law of photometry, it follows that if N and n be the numbers of stars brighter than the magnitudes M and m respectively, $\log N - \log n = 0.60 (M - m)$, or, in other words, there are 4 times as many stars brighter than magnitude x than there are stars brighter than $x - 1$. Prof. Pickering, after a discussion of the validity of his hypotheses, which need not be considered here, puts the formula to the test by counting the stars of different magnitudes in all parts of the sky, as given in various catalogues. The material for discussing the number of stars fainter than the ninth magnitude is meagre, being confined, in fact, to the catalogues of stars near variables, published by Father Hagen, but the results throughout are striking. The factor corresponding to that which multiplies $M - m$ in the above formula, determined from the counts, is .602 when $m = 0.5$; but between this magnitude and 6.5 its value is about .51, which implies that the number of stars brighter than a given magnitude between these limits is rather more than 3 times the number brighter than the magnitude numerically less by unity. For fainter magnitudes the factor gets rapidly smaller, and for 13.0 is .311, so that there are only about twice as many stars (2,000,000) of magnitude 13 and brighter than there are of magnitude 12 (1,000,000). The residuals of the formula are small. That is, the number of stars of any magnitude computed by the formula agree fairly with the numbers actually. In the whole sky there are, according to Prof. Pickering, 6284 stars of magnitude 6.25 and brighter. The number given by the formula is 6339. Using smoothed values of the constants of the formula, it appears that there are 10,000 stars of magnitude 6.6 and brighter, 100,000 of magnitude 8.7, 1,000,000 of magnitude 11.0, and 2,000,000 of magnitude 11.9. Extrapolated values are uncertain, but it is mentioned that the number of stars in the whole sky of the fifteenth magnitude would be about 18,000,000, and these would be seen in a telescope of 15 inches aperture. This, it may be remarked, is about the number seen on the chart-plates of the Astrographic Chart.

SUN-SPOTS OF THE PAST MONTH.—The very large group of spots which was seen on the Sun from October 4 to 18, and again in the next rotation, can scarcely be said to have returned for the third time, but at the beginning of December several spots which formed a striking configuration appeared on the limb in about the same latitude, following the place of the group previously seen by about 40 degrees in longitude. These spots were on the Greenwich photographs from December 2 to 15, and one of them

afforded to spectroscopists some interesting observations of reversal of lines. When, on December 30, these spots in the southern hemisphere had just entered the visible disk for the second time, and, it is worthy of notice, a well-marked normal spot in the northern hemisphere which had been seen at three previous returns was near the central meridian, a sharp disturbance of the magnets occurred at Greenwich. Some warning vibrations happened about noon on Dec. 30, and at 9 o'clock on the evening of the same day the magnets began a series of oscillations lasting until about 9 in the morning of Dec. 31, the amplitude of the disturbance of the declination needle reaching at least 24'.

RAINFALL IN 1903.—The total rainfall recorded at Greenwich during the past twelve months amounted to 35.55 inches. By reference to the table and diagram of rainfall published in our November number, it will be seen that this is an inch and a half more than the greatest annual amount hitherto recorded, namely that of 1852.

A NEW FORM OF GEODETIC INSTRUMENTS.—Sir Howard Grubb and his son, Mr. Rudolf Grubb, with Dr. J. E. Reynolds, have lately perfected some new patterns of surveying instruments depending mainly on the principle involved in the collimating telescope. The rays from a near object placed at the focus of a lens emerge from the lens parallel, and to an eye viewing this object it will appear distant, so that if by some device these rays from this near object can be made to enter the eye simultaneously with those from a really distant object, the two will appear superposed when viewed through a small telescope—in fact, the images of the two objects are in one plane.

It is not an unusual form of this device to make the rays from the near object, when they emerge from the collimating-lens, fall on a piece of plate glass placed diagonally, so that they are reflected to the eye at right angles to their original direction, and through the glass the eye is also able to see the distant object, but the difficulty often occurs that the reflected rays are not sufficiently brilliant.

This difficulty has now been got over by the gentlemen above mentioned, by the adoption, after many experiments, of an amorphous but not crystalline film of sulphide of lead, which can be deposited by a chemical process so as to give the maximum of transparency and reflected power and the minimum of absorption.

The method can be applied to various kinds of instruments. In the Grubb prismatic compass the divisions on a compass card are seen projected on the picture of the horizon, and thus the actual compass-bearing of any point on the distant scene can be at once read off. In another instrument, the graphometer, the image of a scale, the near object, is projected on a staff placed in turn at various salient points of an area to be plotted. By use of a scale of reciprocals the lengths on the staff thus measured give the distances

of these points from the origin of measurement, as well as the directions which are otherwise measured, and thus the area is plotted. The same principle is used also in Sir Howard Grubb's patent gun-sights.

SIR W. RAMSAY AT THE BRITISH ASTRONOMICAL ASSOCIATION.

—The attendance must have been a "record" one at Sion College on December 30, when Sir W. Ramsay was kind enough to attend the Meeting of the B. A. A. and give an address which he entitled "Speculations on Stars and Atoms." We hope to give the substance of this next month in our usual account of the Meeting, but it may be mentioned here that the lecturer gave, among other things, a very acceptable explanation of the cause of the aurora. He suggested that the Sun gives out electrons, many of which find a resting-place in the upper atmosphere of the Earth. An experiment has been tried as follows:—The top of a glass globe containing krypton, which is thought to be present in the upper air, was surrounded by coils of wire, and a current of electricity being discharged through the wire, a circular ring of light, such as would, if it existed in our atmosphere, give the appearances associated with the aurora, was seen, and this gives the clue to the explanation of the phenomenon. When the Earth is in a magnetic condition the electrons forming the light-ring are drawn down to the Earth and so form streamers.

Sir W. Ramsay gave a most lucid and interesting account of the discovery and properties of radium, which was much appreciated by his audience.

THE SHORTEST DAY.—It happened that the recent Winter Solstice fell a few minutes after Greenwich midnight on December 22, and therefore the duration of daylight at places on a meridian a few degrees west of Greenwich was equal on December 22 and 23, so that both of these could claim to be the shortest day of the year, a fact which received due recognition in the daily papers. The solstices in 1903, and of course the equinoxes, fell as late in the calendar as it is possible for them to do: because, first, it was a year preceding a leap-year; secondly, the leap-year is the first of the century, and that century is one whose closing year, viz. 2000, will be a leap-year.

ASSISTANT DIRECTOR OF CAMBRIDGE OBSERVATORY.—The Observatory Syndicate have had under their consideration the fact that Mr. Newall has generously given his services as Newall Observer to the University since 1890. They believe these services are so valuable as to deserve special recognition by the University, and therefore recommend "That the honorary office of Assistant Director of the Observatory should be created and that Mr. Newall should be appointed to the office, it being under-

stood that the office is to lapse at the conclusion of Mr. Newall's tenure." We offer our sincere congratulations to Mr. Newall.

The number of the 'University Reporter' in which the above announcement is made also contains the following:—"It is found that the Transit Circle is in need of certain repairs. The Syndicate are of opinion that these repairs should be undertaken immediately, and that at the same time some minor improvements should be introduced which are now usual in instruments of the first class."

Messrs. ZEISS have kindly furnished us with a small pamphlet descriptive of their new pattern of stereoscopes, some excellent examples of which were exhibited in the library at the last meeting of the R. A. S. and received much commendation. The price of these instruments, some of our readers may be glad to know, is 40 shillings.

THE next Meeting of the Royal Astronomical Society will be on Friday, January 8; of the British Astronomical Association on Wednesday, January 27.

FROM AN OXFORD NOTE-BOOK.

IN December, 1901, I wrote a brief note on the Stereo-comparator, ending with these words: "I gather from Dr. Wolf that the instrument is made by Zeiss of Jena, and costs about £50; and I only wish I had £50 to spend in getting one at once." It would seem that though the days of the fairies are gone, there are very satisfactory substitutes among human beings. An enquiry was soon afterwards made whether the wish was meant seriously, and whether the instrument would really be of use in our work: with a very definite hint that the money would be forthcoming if the reply were satisfactory. In such a case it was important to be quite sure of the facts by actually trying the instrument, and this was not easy; for the number of completed instruments was small and they were all in use at distant observatories. After some delay the hope of getting an instrument on trial had to be given up; but Dr. Max Wolf, of Heidelberg, a fortunate possessor of one of these instruments, kindly volunteered to make the necessary experiments. They proved successful: but the cost of the instrument proved to be much larger than that above mentioned,—with additions and improvements it came to £120 and more. It would have been only natural if the original kind offer had been withdrawn under these circumstances; but it was cordially renewed, and by a fortunate chance

we have secured a completed instrument. It was on view at the rooms of the Royal Astronomical Society from November 27 to December 14; and I gladly here acknowledge the trouble taken by the London house of Messrs. Zeiss in setting up the instrument there in order that the Fellows might see it. It was repacked and delivered in Oxford on December 21, and is now set up in the University Observatory. We hope to show our gratitude to the generous donor (Mr. C. L. Brook, F.R.A.S.) by getting some work out of it at an early date.

THE hypothesis that the expansion of the nebulous appearance round Nova Persei was really due to the expansion of the illumination has hitherto met with a puzzling opposition in some quarters, especially in America. For instance, I was surprised to hear Prof. Newcomb say a few months ago that he considered it more probable that actual matter was being ejected with 10 times the velocity of light than that we were witnessing the spread of illumination. The key to the puzzle is probably afforded by a note which Prof. Newcomb has just published in *A. J.* 550, in which he amends the "method heretofore adopted of estimating the apparent magnitude of the illuminated nebula," which he considers was faulty in "leaving out of consideration the fact that the light sent out by the star in directions near that of the Earth will reach us at an earlier moment than it will if sent out at right-angles to that direction." But where did this fault originate? It is certainly not in Kapteyn's original note, as Prof. Newcomb may see by turning to *Ast. Nach.* No. 3756, where the same form is given to the illuminated surface as is now deduced by himself. The misconception has not been universal, though it is true that computations of parallax, involving further assumptions for which there is no warrant, have been made and allowed to stand without protest—one of them, as Prof. Newcomb remarks, by Kapteyn himself. However, it is a good thing to have an oversight, whether partial or widespread, removed; and we gain, from its having existed, at least a very clear explanation why it should never have been born.

THE new chemical elements seem to have a knack of creating quips. We have all heard how Sir W. Ramsay "ran helium to Earth": and when he was commenting on the fact that there seemed to be no argon in the Sun—"there is lots of helium, where is the argon?" the prompt reply was "It's a' gone!" And now I learn (perhaps late in the day) that when the marvellous properties of radium were explained to a Frenchman, he remarked, "C'est très Curie-ux!"

FROM the *Photographic News* of 1903, December 11 :—

A French astronomer, M. Touchet, who has previously photographed objects by the light of Venus and Jupiter, has now succeeded in obtaining a photograph by the light of Sirius, the most brilliant of all the stars. He used an ordinary camera, but attached to the objective a cardboard tube, to the end of which he affixed a brooch. The light of the star was concentrated by a powerful lens upon the brooch, and an exposure of an hour and five minutes was given. An excellent photograph of the star, with a clear image of the brooch in its centre, was obtained.

WE are indebted to the *Daily Telegraph* of Dec. 18 for the following pieces of wisdom. In one or two of them one recognizes old friends, but friends that one is quite glad to meet again :—

The Sun never sets on British possessions because the Sun sets in the west, and our Colonies are in the north, south, and east.

Cum grano salis.—Although with a corn, thou dancest.

A parallel straight line is one that when produced to meet itself does not meet.

To remove air from a flask : fill the flask with water, tip the water out, and put the cork in quick.

The probable cause of earthquakes may be attributed to bad drainage and neglect of sewage.

Question : Define a circle. Answer : Take your centre and take your distance, and draw a straight curved line. This is a circle, and all lines drawn to it are equal.

Blood consists of two sorts of cork-screws—red cork-screws and white cork-screws.

Question : Define a point. Answer : A point is that which has no magnitude, but only length and breadth ; and there is a point in a circle where all straight lines are equal.

The ends of the parabola will never meet, though, of course, this could be done by making one small enough.

In the United States people are put to death by elocution.

Political economy is the science which teaches us to get the greatest benefit with the least possible amount of honest labour.

Gravity was discovered by Isaac Walton. It is chiefly noticeable in the autumn, when the apples are falling from the trees.

One boy was told to state and prove the relation between the squares on the three sides of a right-angled triangle. He answered thus : "A square is erected on BC, AC, and BA, and lines drawn to certain extremities of the squares formed, then with the help of the fourteen proposition, the fourth, and a few other propositions of the first book, it is shown that $BC^2 = BA^2 + AC^2$." Having completed the task to his satisfaction, he added : "N.B.—It would have been far easier to make the construction and prove the proposition as is done in any book of Euclid, but since you only wish it stated and shown, I have done so."

FROM the *Japan Weekly Chronicle* of 1903, October 14 :—

GAZING HEAVENWARD.

OR A FATAL SHOCK.

Tuesday's eclipse of the Moon resulted in at least the death of one foreigner, and it is appalling to contemplate the number of fatalities that might occur in

the foreign community of Kobe if the practice of gazing at the Moon is persisted in. On Tuesday evening a foreigner went into his garden with the object of looking at the Moon. Having cleared away the stones and snakes in his immediate vicinity, he lay down in a blanket and got up with a cold. From the *post-mortem* examination it was elucidated that the deceased died through laboured respiration and shock, and at the inquest Dr. Frederick Augustus Guppy testified that craning his neck to look at the Moon was the direct cause of the man's death, the deceased having never had occasion to look in that direction since he came to Kobe.

THE following paragraph from one of our weekly contemporaries contains "new matter":—

STAR PHOTOGRAPHY.—Star photography is one of the most tedious operations known. In some cases the exposure of the plate must last for several hours. During all this time both the plate and the telescope must be moved so that the image of the star will be stationary on the plate. The exposure for the star of the sixteenth magnitude is two hours, and only the image of one at a time can be secured unless those adjoining happen to be of the same size.

THE subjoined letter appeared in the *Standard* of 1903, December 8, under the heading "More light on the Fiscal Question":—

SIR,—Perhaps you will allow me to draw the attention of your readers to an indirect potential effect of the Fiscal Reform agitation—viz., the successful restriction of the hitherto unrestricted and continuous dumping of large surplus foreign commodities into our unfortunate "Dustbin" (we used to call the place "Free England," but we have now discarded that "ancient shibboleth"). These commodities were being dumped by a foreign planet (to wit, the Sun) absolutely free of all cost, and consisted of foreign surplus solar light and heat, the home markets of the Celestial Body being, by the force of circumstances, entirely closed to us.

A great Statesman, in May last, startled the "Dustbin" by pointing out the enormous damage done to our workmen engaged in the candle, gas, oil, and electric lighting trades by this scandalous traffic. We had hitherto been too dense to realise the evil without his aid. By way of retaliation, a heavy tax on all windows admitting more dumped daylight than could penetrate a hole measuring one square inch was threatened by the Fiscal Reform agitators. The effect has been most striking, and fully justifies the new policy. The foreign planet has taken the hint, and all this summer has restricted the dumping of its surplus commodities in this country to a minimum, to the great and manifest advantage of our native lighting and heating trades.

It has been estimated for me by skilled statisticians (although I am alive to the fact that figures will be attacked) that if foreign solar dumping could be wholly put a stop to, no less than 5,980,002 additional British workmen would be employed in the electric lighting business alone; and that the profits arising from the comparatively insignificant candle trade, if properly protected by a window tax, would provide an adequate fund of an ample and generous scheme of Old-age Pensions applicable to every man and woman in the country.

I am, Sir,

Your obedient Servant,

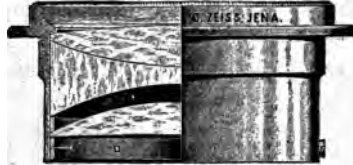
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A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

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No. 341.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 January 8.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker announced that sixty-five presents had been received since the last meeting, and mentioned that there were among them Volume II. of the Annals of the Royal Observatory, Cape of Good Hope (S. S. Hough, Heliometer Triangulation of Southern Circumpolar Area); Harvard College Observatory Annals, Vol. 51 (Photographic Atlas of the Moon, by W. H. Pickering). Also two volumes of the Astrographic Catalogue, one from the Vatican Observatory at Rome, Volume I. of the zone $+55^{\circ}$ to $+64^{\circ}$, the other from the Helsingfors Observatory, which is Vol. IV. of the zone $+40^{\circ}$ to $+46^{\circ}$: in each case this is the first instalment published by these two observatories.

After the formal admission of some new Fellows the *President* said: Fellows will be interested to know that although no formal ceremony of admission of our Honorary Members takes place, their names have been entered upon our roll. It is well to have these names on this list, which includes also the names of our patrons.

Mr. Conrady gave an abstract of his paper on the Chromatic Correction of Object-glasses. He pointed out that the disproportionality in the spectra produced by crown and flint-glass respectively rendered perfect achromatism impossible, and showed that the result of combining the two kinds of glass was that the object-glass had a minimum focus for light of a certain wave-

length, whilst all light of sensibly different wave-length was brought to focus at greater distances from the object-glass. He further stated that it was of great importance when dealing with object-glasses of considerable aperture that the wave-length for which the minimum focus was to be established should be carefully chosen and rigidly adhered to.

The ordinary trigonometrical methods of computation not being adapted to determining this minimum focus in a direct and rigorous manner, Mr. Conrady offered a new method of computing the chromatic aberration, based directly upon the undulatory theory of light, by demanding that the refracted waves of light of a slightly different wave-length shall coincide in the axis and in the margin of the segment transmitted by the object-glass with the refracted wave of the selected wave-length for which minimum focus is to be established.

The mathematical formula expressing this condition was given as

$$\Sigma dn(d-D)=0,$$

the sum to be taken for all lenses constituting the complete object-glass: dn , in any one lens, being the difference of refractive index for two slightly different wave-lengths adjoining the selected one, d the axial thickness of the lens, and D the length of the path of the marginal ray of the selected wave-length through the lens.

To determine the differential refraction dn Mr. Conrady stated that he had found an empirical formula

$$n_{\lambda}=n_0+v_1\lambda^{-2}+v_2\lambda^{-4},$$

where λ signifies any wave-length in the visible spectrum, n_{λ} the refractive index for that wave-length, n_0 , v_1 , and v_2 constants to be determined from three given indices, of sufficient accuracy and greatly superior to the three terms of Cauchy's theoretical formula

$$n_{\lambda}=n_0+v_1\lambda^{-2}+v_2\lambda^{-4},$$

as well as to Schmidt's empirical formula

$$n_{\lambda}=n_0+v_1\lambda^{-1}+v_2\lambda^{-4},$$

which latter gave the most concordant results obtainable with two whole-number powers of λ .

The President. I will now ask for any remarks on this interesting paper by Mr. Conrady. It is seldom that we have a paper of this kind from one who is able to combine theoretical with practical knowledge in the way he seems to have done.

Mr. Dyson. There is one interesting point in Mr. Conrady's paper to which he referred in the summary he gave to the Meeting, and that is the part in which he gives the index of refraction in terms of the wave-lengths in modification of Cauchy's

formula. Perhaps Mr. Conrady will give us further details as to the accuracy of his formula and the range of spectrum through which it applies.

Mr. Conrady answered the question by a demonstration on the blackboard, and, in reply to a question by Mr. Bryant, stated that his method did not involve any assumptions; when his condition had been satisfied for a selected wave-length the differences of focus for light of sensibly different wave-length necessarily followed.

Mr. Whittaker thought it would be interesting to learn between what range of wave-lengths the new empirical formula was verified, because it was well known that Cauchy's formula failed beyond a limited range.

In reply *Mr. Conrady* stated that he had verified his formula for the visible spectrum only, between the A and the G lines, and that it rendered the indices throughout that range within a few units of the fifth decimal.

The President. I happen to know we have a visitor amongst us who has himself ground lenses. I do not know whether Mr. Metcalf would like to say anything on the subject.

Mr. Metcalf. I must confess that I have studied the mathematics of object-glasses and have done a little in this direction. My trouble has never been with the mathematical theory of dispersion, but with the practical correction for spherical aberration, which is a very much finer quantity, and therefore much more difficult. Prof. Woltan has experimented with the 12-inch which I tried to grind, and though, of course, I do not like to say very much about it, I must admit that it did not come out very accurately, and I think that the trouble was in the values of the indices of refraction; but, so far as my practical knowledge goes, the rule of thumb "amounts to considerable" at the end of any operation. I know that in the large lenses which they grind now they do things very much by trial. They compute as accurately as they can what the curve should be, and then having made this it is ground and polished as seems necessary. I think one of your own men said that is what it comes to in the end. That does not seem to be true of Mr. Conrady and of his work, although, not having seen any of his lenses, I am not in a position to speak about it with certainty.

The President. I am reminded of an incident which may be interesting, namely, that my predecessor at Oxford, Prof. Pritchard, occupied a great deal of his attention at one time on the construction of certain tables which should give at a glance the proper radii of curvature for the surfaces of lenses of known refractive index; but these tables were never used. Talking to an eminent optician, I mentioned this fact, and he said there were reasons of this kind why such tables could not be utilized. Supposing you know that the radius of a certain surface should be 35 inches and you have a grinding-tool of 30 inches. You cannot afford to

make a new tool of 35 inches radius and you use 30 inches and make the proper correction by what is called "rule of thumb." I am sure we are glad to have this paper on a line of work which does not usually come before us.

Mr. W. Ellis read a paper on "The Aurora and Magnetic Disturbance." He said that in papers appearing in the 'Monthly Notices' for December 1899 and June 1901 he had shown that the frequency of magnetic disturbance at Greenwich varied with sun-spot frequency, and, further, that in the former there existed an annual inequality having maxima at the equinoxes and minima at the solstices to which there was no counterpart in the progression of sun-spot frequency, which annual inequality was similar in form to that of the aurora in the same locality. He now desired to carry the matter a little further. In a recently published work J. F. Schroeter has collected and discussed the Norwegian and Swedish observations of the aurora made through a long series of years, and, finally, added a table in which, for five latitudinal regions, there is given the comparative frequency of the aurora in each month of the year. And in a paper by Mr. Mossman, in the Journal of the Scottish Meteorological Society, there is given corresponding information for the N.E. of Scotland, Edinburgh, and London. To these were added curves showing the frequency of magnetic disturbance at Greenwich and Paris. Viewing the similarity of the auroral and magnetic curves in lower latitudes, and the disappearance of the equinoctial maxima and winter depression in the aurora as higher latitudes are approached—all being merged into one pronounced winter maximum—the question arose whether if an observatory existed in a high latitude, would the annual frequency of magnetic disturbances become changed as does that of the annual frequency of the aurora? The persistent character of the annual inequality in frequency of magnetic disturbance was shown, not only in years of maximum frequency of sun-spots, but also in years near minimum frequency of sun-spots, being traceable in years of very moderate disturbance. The inequality is also greater as the amount of disturbance is greater. The paper contains also some discussion of the relation between the frequency of the aurora and that of the $11 \pm$ year sun-spot period in northern regions.

Mr. E. W. Maunder then read a paper by *Mrs. Maunder* on a suggested connection between Sun-spot Activity and Secular Change in Magnetic Declination. The paper pointed out that since sun-spots were first discovered, in the year 1610, there have been two striking cases of prolonged solar quiescence. The first was the most pronounced, and lasted from about the year 1640 to about the end of the seventeenth century, during which period but a single spot now and then appeared, even at times when the Sun should have been most active. The "eleven-year" cycle seemed obliterated, and an almost unbroken solar calm prevailed. Auroræ were notably

absent. The second period of solar quiescence, at the beginning of the nineteenth century, was remarkable but not so complete. The "eleven-year" cycle could still be plainly traced, but for several succeeding maxima the solar activity was much below the normal; and this was emphasized by the great activity that preceded and followed the period of semi-quiescence. These two periods of solar calm appear to have been answered on the Earth by corresponding periods of absence of auroræ. There was thus sympathy between solar quiescence and terrestrial magnetism. When the movement of the north "magnetic pole" was considered, it appears that at these two very epochs it was passing through two critical points of its path: the first when it was at its nearest approach to the geographical pole; the second when it was at its greatest elongation from it.

Mr. Maunder continuing said that his own paper dealt with the particulars of certain magnetic storms from 1875 to 1903, and their association with sun-spots as recorded at the Royal Observatory, Greenwich. He said that the paper which had been read before the Society in December 1899 by Mr. Ellis had classified magnetic disturbances under five heads, those in which the extreme amplitude in movement in declination was over a degree, which he had termed "great" disturbances, being the most considerable. Taking only the "great" disturbances from 1875 up to the present time, it would be found that these were 19 in number, and he had also taken out the particulars of the 19 largest spots at Greenwich during that time. Of the 19 great magnetic storms, it was found that 7 coincided with the passage across the centre of the disc of one of the 19 great spots; 1 of the storms coincided with the return for a second time of one of those great spots—it was reduced in size, but still considerable; 9 of the 19 storms coincided with a large group of spots, and 2 with the return of a spot which had been large in former appearances, but in a smaller phase; so that all of the 19 coincided with the actual presence of a large group of spots on the Sun or else with its return. Bearing in mind that the twenty-nine years to which the paper referred covered a period of over 10,000 days, and that these 19 storms included all the largest storms, those not included being of a distinctly smaller character, this correspondence was very striking indeed. Taking the whole comparison together, there were four points which seemed to come out. First, there was a real connection between large sun-spots and great magnetic storms. This was shown (*a*) by the fact that in every case a "great" storm was synchronous with the passage of a large spot or with its return; (*b*) by the exact correspondence of the very greatest storms to the time of visibility of the very greatest spots; (*c*) by the fact that there were no considerable storms in the interval between the two great spot-groups of 1898 September 3–15 and 1903 October 4–18. The appearance after an interval of more

than five years of a "very large" group of spots was answered by a magnetic disturbance greater than any which had occurred in the interval. Secondly, there was a real, but only rough, connection between the size of the spot and the intensity of the storm. This was shown by the correspondence of seven out of the nineteen greatest storms to seven out of the nineteen greatest spots in a period of over twenty-nine years. The proportion of correspondence in so long a period was far too high to be due to accident. Of 19 groups shown in Table II., the 9 largest all coincided with a "great" or "active" storm. Thirdly, the area of the spot-group was not by any means an exact index of the degree of intensity of the magnetic disturbance. Fourthly, "great" storms did not begin indifferently at any time of the passage of a great spot across the disc of the Sun, but during the period of 5 days, beginning 34 hours before the centre of the spot-group reached the central meridian and ending 86 hours after that time, the mean time being 26 hours after the spot reached the central line.

Mr. Maunder then read a note (communicated by the *Astronomer Royal*) on the "Great" Magnetic Storms 1875-1903, and their association with Sun-spots. The facts of the preceding paper were, so far as they went, in entire agreement with the facts tabulated in the important paper by the Rev. W. Sidgreaves, read before the Society on 1900 December 14. The difficulty was in their interpretation. It had been urged that if there were a real direct connection between a particular great sun-spot and a great magnetic storm, then that connection should always show itself. Every great spot on the Sun should be answered by a great magnetic storm here; every great magnetic storm should be synchronous with the appearance of some great spot; and, further, the violence of the storm should bear a definite relation to the extent of the spot. This relation did, indeed, hold good if we considered only the average of a number of instances; it did not hold good universally in every individual case. An example was in the immediate recollection of everyone. The sun-spot which crossed the central meridian on 1903, October 12th, was one of the six or seven largest groups seen during thirty years. The magnetic storm which occurred at this time was an "active" one, not a "great" one. The sun-spot of October 31 was a large one, but not of extraordinary dimensions—less than one-third the size of that of October 12. Synchronous with its passage over the central meridian was the greatest magnetic storm that has been recorded for thirty years, perhaps the greatest that has yet been recorded at Greenwich. The natural expectation would have been that the reverse should have been the case—that the greater spot should have corresponded to the greater storm, and the smaller to the feebler one. Yet such an expectation, however natural, involved a number of assumptions, for some of which we had very little warrant. It assumed, for example, that the efficiency of a spot was greatest at the moment of its attaining

its greatest area. More important still, it assumed that its influence was equally great in all directions; that there was nothing like *direction* in the forces or emanations which proceed from that disturbed region of the Sun of which the spot is the visible indication. Yet this assumption was founded upon no sufficient evidence, even if it were not a wholly gratuitous one.

Mr. Maunder then urged that the existence of the long coronal rays indicated a solar action in certain definite directions, and that if we suppose that the effect of a solar disturbance travelled outwards in somewhat the same manner as these long coronal rays, this would remove the ground of the difficulty. The intensity of any magnetic storm due to a solar disturbance would then depend upon two factors: first, the actual magnitude of the disturbance itself; and, next, upon the distance of the Earth from the direction of maximum effect. We should find, as we actually do, that when the average was taken of a large number of cases, the frequency of magnetic storms and their intensity would correspond to the size of the solar spots; but at the same time we should also find, as we do, that there would be a wide margin of irregularity in special instances. It is in perfect accord with this suggestion that we actually find at the moment of commencement of the nineteen great storms examined that the most important spot on the Sun was always found within a restricted area on the surface. If the influence of the spot were exactly equal over the whole sphere of which it was the centre, it is difficult to understand why this relation should have shown itself.

Mr. Bryant. I should like to ask Mr. Ellis whether any consideration has been taken of the length of the night at different parts of the year; and also whether the weather has been taken into consideration. I should think that the effect of minimum at Greenwich in winter would be quite as likely to be due to a bad sky as to any other reason. I do not know whether that has been taken into consideration.

Mr. Ellis said that the autumn and spring maxima of the aurora approached each other in higher latitudes, the winter depression at the same time diminishing, until the three coalesce in one mid-winter maximum at the darkest part of the year. The influence of daylight and of atmospheric circumstances in winter at Greenwich are matters not readily discussed in a few minutes; but observers of auroræ are quite alive to these things, which do not in our latitude conceal the broad facts.

The President. There are at least two interesting points raised in these papers. The suggestion made by Mr. Maunder—namely, that some directive characteristic in the Sun's activity might be responsible for the apparent failures of connection between sun-spots and magnetic storms; and Mr. Ellis's clear demonstration of the advantages of putting up an observatory at the North Pole. I am sure we shall thank these gentlemen for their suggestions;

and I hope the suggestion made by Mr. Ellis will soon be carried out. If Mr. Newall will take the Chair for a few moments, I would like to say something about a paper of my own.

(*Mr. Newall* then took the Chair.)

The President said that the paper he referred to was not on the printed list because it was being printed in the 'Memoirs.' It concerned the variable-star observations made for many years at the Rousdon Observatory under the direction of the late Sir Cuthbert Peck. During his fatal illness Sir Cuthbert asked me if I would see to the publication and discussion of these observations; and this duty I was very glad to undertake. The work has taken a great deal of time—more than I expected; and although it has been going on for two or three years, it is only now approaching completion. There are one or two points of general interest. First of all there is the study of systematic errors of observation, of which I will give one illustration. In making comparisons of the difference of brightness between two stars the first thing to be done is to convert the "steps" of the observer into magnitudes. At Rousdon comparisons were made of the "variable" with five other stars every night, and instead of "steps" of one or two tenths of a magnitude being used, several whole magnitudes were sometimes recorded. This gave, on first appearance, a roughness to the observations, and I was a little doubtful about their value, so that I consulted Prof. E. C. Pickering, who, however, reassured me, so that I proceeded with the investigation and found the curious result that the estimated magnitudes could be converted into a true scale by *subtracting* a nearly constant quantity, and *not* by multiplying by a factor. [Diagram thrown on screen.] I will pass now to the more interesting part—the results, and I will take as a specimen the star *T Cassiopeiæ*. A suggestion for the exhibition of results in these cases, which I put forward rather diffidently, is that taking a typical curve of this kind [Screen] the results should be grouped in 12 periods, or some such number, and then the differences from the curve tabulated as a series of residuals. I show here a table of 12 columns giving residuals at these 12 epochs, and by a glance one can tell what the star has been doing, whether it has been keeping to its period or not. But now comes the important thing—when there are anomalies, are they due to the star or the observer? It may seem rather a bold suggestion on my part that when there is a residual of a whole magnitude the observer could possibly be at fault, and I only make it because on comparing two sets of observations they did not confirm one another. [Screen.] If this is the case, it seems more probable that the star is regularly periodic, and that the observer makes occasional diversions. Then another point I will bring before the Fellows is a method for quickly determining the period of any given star from these residuals. Many of them seemed to have a constant

period, which we may determine from any one of those columns of residuals by a very simple rule. The corrections to the adopted period from the different columns are, you see, very accordant, and they give a mean correction of about $4\frac{1}{2}$ days. Those are the main points of interest at present.

Mr. Newall, having invited remarks upon the paper, said:—I am sure we all owe a debt of gratitude to Prof. Turner for having undertaken a work of this kind and for having brought it before the Society. We should all be interested if Prof. Turner could give us an idea as to when he expects the reductions to be completed.

Prof. Turner. I am trying to complete them every day. I hope they will be completed within the next week.

(*Prof. Turner* again took the Chair.)

Mr. Tyson Crawford then explained an arrangement he had produced for improving the eyepiece of "finding"-telescopes. Instead of the well-known cross wires, he placed a ring in the eyepiece, which would be found of sufficient transparency to allow an observer to see the star in its transit across the ring. Another advantage he proposed was that the eyepiece should give a very large field—quite double that of the ordinary finder—which would be a very great advantage in picking up a star and bringing it to the centre. He also exhibited a sketching-board to be attached to the telescope, on which the image of the Sun could be projected. This was 9 inches square, with a curtain round it to cut off extraneous light and make the detail clearer. An ordinary solar diagonal eyepiece was to be used with this, and a power of 50 was a very convenient one. It could be moved nearer or further from the eyepiece as required, and one could adjust the position until the best results were obtained. With a $3\frac{1}{2}$ -inch telescope a disc of $3\frac{1}{2}$ to 4 inches diameter could be obtained.

The President. With regard to the question of the reproduction of astronomical photographs, the point to be discussed may be put very clearly by the reading of a brief note which Mr. Wesley has written. The question arose *à propos* of Mr. Ritchey's slides and slides from the Yerkes Observatory generally. It has been frankly announced recently that these beautiful slides, which we have so often had the pleasure of seeing on the screen, were prepared in a special manner, and this has excited some adverse criticism. In such a case it is always better to have a free and full discussion and to throw light on the whole matter. As a preliminary, Mr. Wesley will tell us the result of his own examination of the negatives.

Mr. W. H. Wesley then read a note on Mr. Ritchey's Photographs of the Andromeda Nebula. At the Meeting of the Society in March 1903, there were exhibited a series of photographs of nebulae taken by Mr. G. W. Ritchey with the 24-inch reflector of

the Yerkes Observatory. The photographs were remarkable as showing in the Andromeda and Orion nebulae the detail both in the bright central portions and in the faint outlying parts, and the question was raised as to whether some selective process of development or printing had been employed.

In the 'Monthly Notices' for May 1903 (vol. lxxiii. p. 395) appeared a letter from Mr. Ritchey, fully explaining the process he adopted in developing astronomical negatives. Mr. Ritchey further says that "in the case of such objects as the Orion and Andromeda nebulae something more is needed if the detail in the bright central parts and the faint extensions are both to be shown on the same positive. One negative is reserved precisely as developed by the above method. For transparencies and lantern-slides a second negative is exposed in the telescope, developed as above described, and the parts which are so dense that they cannot be printed are reduced locally by the use of a very weak reducing-solution. Much time and care are given to this, and an attempt is made to keep the relative brightness of the various parts the same in mind, though not in degree, as in the untouched negative, which is constantly used for comparison during the process."

The two beautiful transparencies, therefore, which Mr. Ritchey presented to the Society, and which were shown at the March Meeting, were taken from negatives which had been locally reduced. The advantages of showing on the same plate the detail of all parts of the nebula are sufficiently obvious. At the same time, while the utmost care was evidently taken with the reduction, it seemed very desirable to compare these photographs with entirely untouched negatives, to find if any false effects or spurious detail had been introduced by the process.

A short time ago Mr. Ritchey sent, through Mr. Hinks, two untouched negatives of the Andromeda Nebula, so that, with regard to this nebula, the opportunity was afforded for such a comparison. The negative from which the enlarged transparencies were made had an exposure of $4\frac{1}{2}$ hours, and as one of the untouched negatives had been exposed 4 hours, it was particularly suitable for the comparison. The detail of the nebula is so complex that I principally concentrated my attention upon the finer detail immediately surrounding the great central condensation, as this was most liable to be affected by the local reduction. I examined with special care the edges of the dark rifts where the bright bands are broken up into very minute flocculent patches. The result of my examination is that I have found nothing on the transparency that is not on the negative. I specially looked for any edge of greater brightness which might mark the limit which the reducing solution reached, but can find nothing of the kind. I can, therefore, only conclude that the process of local reduction has been so carefully carried out that no spurious detail has been produced in the negative from which this transparency was made.

Since the prolonged examination of small details renders one extremely liable to overlook larger and more general effects, I have repeatedly broken off my examination and resumed it on other occasions, but the results have always been the same. Mr. Hinks and I have also together compared the photographs, but we both came to the conclusion that whatever was in the transparency could be found in the untouched negative.

I may add, although it is not quite the point about which the note is written, that I could find nothing in the negative which is not in the transparency; in other words, that the transparency seems to bring out everything in the negative. It seemed to me, and I think to Mr. Hinks too, that the process had produced a most marvellous picture from the negative. I suppose we might assume the negative sent us for comparison was one taken in the ordinary course, and that the negative selected for the transparency was a particularly good one chosen for the purpose.

The President. I would invite discussion on the subject, and I would suggest that we should hear, in the first place, somebody who has an adverse criticism to make. Perhaps, however, I had better remove that limitation, and throw the discussion open.

Mr. Hinks. I should like to add my testimony to what Mr. Wesley has said with regard to these photographs. There certainly has been a certain amount of criticism of them through their having been sent out without any mention being made that they had been locally reduced. Of course, it was simply because Prof. Hale and Mr. Ritchey thought there could not be any objection taken to the process that they did not mention it. But criticism there has been, and I think we ought by this time to be able to arrive at an understanding as to whether pictures which have been treated in the way Mr. Ritchey has treated them shall be accepted or not accepted. A halfway course seems very undesirable. Under the guidance of Mr. Wesley I had the pleasure of going over all the points with him, and I can fully corroborate his statement that it is impossible to find anything upon that big positive which is not upon both of the original negatives which Mr. Ritchey sent, and in sending which he said: "They are good average negatives, but they are not my very best." I think it is equally striking that everything that is to be seen on those negatives is to be seen on the positive which Mr. Ritchey has made, for it is well known that some people have to emphasize the fact that there is a great deal more on their original negatives than is to be seen on the positive produced from them. Therefore the fact that everything that is on the negative is to be found on the positive is a very considerable point. It seems to me the power this process has of making available practically everything that is on the original negative is one which should not be lightly thrown away. It is not the least satisfaction to hear from a man that there is a great deal more in the original negative than in the reproduction, and it does seem, so far as one can judge from

the most careful examination, that it is impossible to point to anything that can be objected to at all in the wonderful result that Mr. Ritchey has shown. But it is true that when local reproduction is recognized, the faith you put in the picture depends upon your faith in the skill of the man who makes it.

It has been suggested that, to save any possible misconception, it would be well if there were a general understanding that when any treatment of the sort takes place the fact should be stated on the label; and it is possible, if the Meeting this evening accepts this suggestion, that everyone who sends out photographs will adopt it.

Dr. Spitta. This method of local reduction is a very old one, and there is not the slightest difficulty in working it save for one objection, and that is to prevent a line of demarcation where the reducing-agent commences and where it ends. This class of negative would favour the non-discovery of such positions, and so may be said to lend itself to the process. In dealing with a plain surface like the background of a photograph, to thin down the density from the centre to the circumference is exceedingly difficult without showing the marks of the reducer. There is another and far better plan which is mostly adopted, especially for rendering negatives fit for enlargement, so as to get out all the details in the densest portions, and that is by differential printing. This is effected by exposing the over-dense portions for a greater time than those parts of the negatives which are thin, and is easily done by shading such too-transparent portions, taking care to move the mask a little every second or so during the exposure. With a little practice this plan can be adopted without showing any line of demarcation, about which I have spoken, as inevitable with the thinning method. I should much like to see the plan tried on this type of negative and subject.

Mr. Hinks. With reference to what Dr. Spitta has said, I happen to have in my pocket three photographs of the Orion Nebula which Mr. Wilson, of Daramona, allows me to show to-night. The first is the original negative, full of detail; the second is a direct-contact positive made in the ordinary way, showing no detail in the centre; the third is a positive made by gradually screening off the outlying parts, and thus printing the centre more deeply. The result is full of detail, and it will be interesting to compare it with Mr. Ritchey's picture of the Orion Nebula.

Dr. Spitta. If I may be allowed to add to my previous remarks, in making reproductions of these difficult negatives, when a *new negative* is required rather than a simple lantern-slide, it is important to remember to take the first copy on a quick plate, and the second, or new negative, also upon a quick emulsion. This is for the purpose of reproducing all the fine gradations of the original. If you employ a slow plate more contrast is obtained, it

is true, but you lose much of the fine detail. It has often struck me that perhaps such mistakes may very frequently account for the complaint so often heard that the copy on a lantern-slide does not give anything like the fine detail of the original; if anyone will intelligently make the comparison the difference in results is very striking.

The following papers were announced and partly read:—

P. H. Cowell. "Transformation of Hansen's Tables."

H. H. Turner. "Note on the Use of Long-Focus Mirrors for Eclipse Work."

Rev. T. E. Espin. "New Double Stars detected with the 17½-inch Reflector during the Year 1903."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Jupiter, 1904-5."

W. F. Denning. "The Rotation Period of Saturn in 1903."

E. W. Maunder. "The 'Great' Magnetic Storms, 1875 to 1903, and their Association with Sun-spots, as recorded at the Royal Observatory, Greenwich." (Communicated by the *Astronomer Royal*.)

Mrs. E. W. Maunder. "Suggested Connection between Sun-spot Activity and the Secular Change in Magnetic Declination."

A. E. Conrady. "On the Chromatic Correction of Object-glasses."

William Ellis. "The Aurora and Magnetic Disturbance."

E. W. Maunder. "Further Note on the 'Great' Magnetic Storms, 1895 to 1903, and their Association with Sun-spots."

W. H. Wesley. "Note on Mr. Ritchey's Photographs of the Andromeda Nebula."

The following gentlemen were elected Fellows of the Society:—

Henry John Wolverton Brennand, M.A., M.B., Ch.M., F.C.S., Government Statistician's Department, and 203 Macquarie Street, Sydney, New South Wales.

Albert Edward Garrett, F.R.G.S., 127 Lothair Road, Finsbury Park, N.

Joseph M. Harvey, 77 Clousdale Road, Upper Tooting, S.W.

John George Hatchard, C.S.A., Railway Works, Pretoria, Transvaal, South Africa.

Rev. Gustavo Heredia, S.J., Stonyhurst College, Blackburn, Lancashire.

Herbert Kitchen, Marshall Square, Johannesburg, Transvaal, South Africa.

George Aimer Russell, M.A., B.Sc., 29 Glebe Road, Kilmarnock, Scotland.

Frank Herbert Shaw, Burlington House, Huddersfield.

The following Candidate was proposed for election as a Fellow of the Society:—

Percy Morris, Holmwood, Camborne Road, Sutton, Surrey (proposed by H. F. Newall).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1903 December 30.

President: S. A. SAUNDER, M.A., in the Chair.

Secretaries: A. C. D. CROMMELIN, B.A., and J. G. PETRIE.

THE names of four Candidates for Membership were read and passed for suspension, and the election by the Council of six new Members was unanimously confirmed.

The President, in introducing Sir William Ramsay, reminded the Meeting that eight years ago he, Sir William, had delivered a lecture on Helium before the Association; he was now going to put them under further obligation by addressing them on the subject of Radium. The title of the address which he had promised was "Some Speculations regarding Stars and Atoms."

Sir William Ramsay commenced by referring in some detail to the discovery and identity of terrestrial and solar helium; he then proceeded to give a very interesting account of a series of experiments that he had made into the nature of the aurora borealis, which he considered that he had to a certain extent solved. According to the Swedish physicist, Arrhenius, it was probable that there were corpuscles, similar to those evolved from radium, continually being shot out from the Sun. These corpuscles were extremely minute electrified bodies, and they impinged on the upper layers of the atmosphere and electrified it. A reasonable explanation of the aurora was that a ring of gas in the upper atmosphere around the poles was illuminated by the electric discharge and drawn down by the magnetic field of the Earth, and appeared in the form of streamers. The spectrum of the aurora was identical with one of the prominent lines of the gas Krypton. The power of krypton to give out light was very much greater than that of any other gas, and therefore its spectrum, rather than that of other more abundant gases, was always conspicuous in the aurora.

Turning then to the subject of radium, Sir William pointed out that the subject of radiation had greatly developed within the last few years, owing to the discovery by M. Henri Becquerel of uranium rays. Madame Curie took up the subject, and discovered in uranium ores a substance of greater activity than uranium, to which she, being a Pole, gave the name of Polonium; but the most remarkable discovery by M. and Mme. Curie was that of radium in the residue of the pitchblende, after the extraction of the uranium.

With the help of Prof. Giesel, the German chemist, it had been proved that radium was a million times more active than uranium.

Radium, said Sir William, emitted three kinds of rays, known as the α , β , and γ . The α rays, which had been investigated by Prof. Rutherford, were associated with atoms of matter, each about twice the weight of a hydrogen atom. He (the speaker) and Mr. Soddy had investigated the α rays, because Prof. Rutherford and Mr. Soddy had thought that the atomic particles thrown out in the rays evolved from radium had a density not far removed from that of helium, and might conceivably consist of helium. This they had proved, as the spectrum of helium gradually developed in a tube into which only radium emanation had been put. The β rays, unlike the α , were particles with great powers of penetration; and the γ rays emitted vibrations of ether.

Sir William then proceeded to suggest the possible analogy between atoms and the stellar system. It was conceivable, he said, that an atom was something like the stellar system; but whether there was anything similar to a stellar sun he did not know. It was a curious fact that there appeared to be a sort of limit to two natural phenomena: one was to the size of a star, and the other to the size of an atom. The heaviest atom known was that of uranium; and it appeared reasonable to suppose that the atom was composed of a large number of electrons revolving round one another, something like a swarm of meteorites, that the atom became unwieldy, and that the electrons were shot off and pursued their way through space. They had thus some sort of foreshadowing of the possibility that there were some atoms too heavy to be stable. They had reached the acme of atomic weight. It would, of course, be going too far to say that the atoms were falling apart, as it would be to say that our solar system was stable; but the cases were on all fours.

Father Cortie asked Prof. Ramsay whether the α rays, the β rays, and the γ rays were a different substance from the emanation.

Sir William Ramsay replied that the β rays appeared to consist of electrons, the γ rays of ether vibrations, and the α rays appeared to be evolved from atoms of radium emanation.

Father Cortie. Did I understand you correctly that first of all radium parts with an emanation, and that the emanation gives out α , β , and γ rays? or are α , β , and γ rays the same thing as the emanation?

Sir William Ramsay said the emanation was a gas; it decomposed, and gave off α , β , and possibly γ rays. When enclosed in a tube the gas shone for about 28 days, and a deposit was found on the surface of the glass which disappeared in about four hours. What the deposit was no one knew. It was some form of matter or electrons which, when shot on to the glass, had the power of discharging electricity.

Rev. T. E. R. Phillips. I suppose α rays lose electrons?

Sir William Ramsay. Possibly.

Rev. T. E. R. Phillips. Does the initial energy of radium come from the disintegration of the atoms?

Sir William Ramsay. That is supposed to be the case.

A Member, referring to the contention that in a few million years the heat of the Sun would have disappeared, asked if such a theory would be upset by the discovery of radium.

Sir William Ramsay said that a good deal of correspondence had recently taken place in *Nature* on the subject. If the Sun consisted wholly of radium it was conceivable that the radiations would never reach the Earth, but would be stopped by the atmosphere.

Father Cortie stated that he had carefully compared the wave-lengths of the lines of the spectrum of radium as determined by Sir William Crookes with the wave-lengths of the lines of the solar spectrum and found no instance of the reversal of a radium line in the solar spectrum. That was presumptive proof, though, of course, not an absolute proof, of the non-existence of radium in the Sun. If radium were in the Sun in such quantities as to account for his annual output of heat, one would expect that it should manifest its presence by the reversal of at least some of its lines.

The President inquired whether radium could possibly exist in the form of a gas.

Sir William Ramsay said it had not yet been investigated, but he did not think there was any reason against its being made gaseous at a sufficiently high temperature.

The President remarked that what struck him most was the enormous complexity of the subject. It had a very close connection with astronomy, and might compel them to revise a great number of their theories. He asked the meeting to pass a cordial vote of thanks to Sir William Ramsay for his address.

The motion was carried with acclamation.

Sir William Ramsay, in acknowledging the vote, said he had really attended their meeting in search of information, and he would, therefore, have been glad if there had been a little more criticism of his remarks. He was not quite sure of some of his statements, and he would have liked to have had the views of astronomers regarding them.

Father Cortie, in presenting the Tenth Report of the Solar Section, for the year 1900, explained that it consisted of two parts, the first dealt with sun-spot statistics, and the second with solar prominences. The daily range of the declination magnet, as well as the magnetic state for each day, was also given, so as to enable comparisons to be made between the state of solar activity and terrestrial magnetic disturbances. The total number of observations of the solar surface obtained for separate days was 339. Only two observers had contributed observations of the solar prominences. It was very desirable that more observers of solar prominences should join the Section. The form of the report

was similar to that of 1899, so that the life-history of each spot-group could be ascertained by reading the separate columns vertically, and the state of the solar surface as to number of spots and faculæ, the disc-area of the spots as well as the magnetic state for each day, could be found by reading the columns horizontally. The solar activity during the year 1900 was approaching a minimum; the spots were relatively small and short-lived, and were soon broken up by the appearance in them of bridges. In conclusion, Father Cortie referred to the question of the connection of solar phenomena and terrestrial magnetism. Magnetic disturbances were attributed by some to the action of sun-spots, by others to that of prominences and also to the faculæ. But it seemed to him that the connection, which was not necessarily one of cause and effect, was not to be predicated of any one of these separate phenomena, but to a greatly disturbed state of the solar surface and surroundings which was answered to by magnetic disturbances on Earth. The several phenomena on the Sun were not isolated, but intimately connected. This was shown by the strict accordance of the curves for spots, faculæ, and profile area of the prominences. They were quite similar to one another and also to the curve of magnetic diurnal range of the declination needle. It seemed to him that the cause was to be sought in something that acted sometimes on the Sun, sometimes on the magnets, and sometimes on both Sun and magnets.

Mr. Crommelin read a paper entitled "The Recent Sun-spots," by *Mr. W. J. Macdonnell*, President of the New South Wales Branch.

Mr. E. W. Maunder read a letter from *Mr. Lowell*, in which he adhered to all his contentions, not only with respect to the "Canals" of Mars and the limits of vision for straight lines, but also as to the reality of the markings which he had drawn as existing on Venus and Mercury.

The President said that as *Mr. Lowell* stuck to his guns they would no doubt have an opportunity of discussing the subject on a future occasion.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

THE Annual General Meeting of this Society was held on Wednesday, January 20, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker*, President, in the Chair.

The Report of the Council, which was read by the *Secretary*, described the work carried out by the Society during the past year, and showed that there was an increase in the number of Fellows.

One remarkable feature was that out of the 13 deaths reported, 4 of the Fellows had reached the great age of 92 or more years.

The Symons Gold Medal for 1904 was awarded to Hofrath Dr. Julius Hann, of Vienna, in consideration of his eminent services to the science of Meteorology. Count L. Széchenyi, First Secretary to the Austro-Hungarian Embassy, attended and received the Medal on behalf of Dr. Hann.

The President in his Address dealt with the present condition of Ocean Meteorology, and began by referring to the early workers in Meteorological Science—Lieut. F. M. Maury in America, and Admiral R. Fitzroy in England; also to the address on the same subject delivered to the Society by Dr. R. H. Scott, F.R.S., in 1886. He then sketched the present state of our knowledge, illustrating his remarks by numerous maps. He reviewed the meteorological work of different nations, pointing out the energetic action of the United States in particular, and of Germany and England. He regretted the want of liberality shown by the Government in affording financial aid for the development of this important science, and in conclusion he urged the necessity of interesting the youth of the country in the matter by making it a special subject of school and college curriculum.

At the conclusion of the Meeting the Officers and Council for the ensuing year were elected.

A Photographic Atlas of the Moon.*

It is now more than sixty years since the first photograph of the Moon was obtained by J. W. Draper in March 1840, and during those sixty years there is no celestial body towards which the photographic camera has been more frequently turned. But whilst the result has been that we have innumerable photographs of the Moon, it has been left to that enterprise which we have learnt to associate with the names of the Harvard College Observatory and its energetic Director to produce the first complete photographic atlas. The story of Mohammed and the mountain has been illustrated once more, for whilst the astronomers at Melbourne, Lick, Paris, or Yerkes have waited until the Moon has presented itself to them under favourable conditions of illumination and of atmosphere, Professor Pickering, thanks to the generosity of two anonymous donors, found himself, early in 1900, able to organize an expedition to a more suitable climate for the express purpose of photographing the Moon. How great the atmospheric difficulties are at the fixed observatories may be gathered from the fact that

* Harvard Observatory Annals, vol. li., Cambridge, Mass., 1903.

'The Moon. A Summary of the existing Knowledge of our Satellite, with a complete Photographic Atlas.' By W. H. Pickering. One hundred illustrations. Doubleday, Page, & Co., New York. Price \$10.

M. Loewy tells us that, after photographing the Moon on every possible occasion for four years, he found that there were not more than ten nights on which he was able to obtain really satisfactory negatives. An expedition from Harvard to the island of Jamaica, during which a 5-inch telescope had been used at five different stations on the island, had shown that the atmospheric conditions during the summer months were particularly favourable, and accordingly it was to Jamaica that the Harvard expedition was sent out at the end of 1900 under charge of Professor W. H. Pickering.

The equipment marked what was, in this branch at all events of astronomical photography, a new departure. It consisted of a photographic O.G. of 135 feet 4 inches focal length and 12 inches aperture, to be fixed in one position, and fed by a plane silver-on-glass mirror of 18 inches diameter mounted as a heliostat, the rotation of the image being compensated by a corresponding rotation given to the plate-holder. The location ultimately selected was at Mandeville, in $18^{\circ} 1' N.$ latitude and $5^h 10^m 2^s.5$ W. longitude, at an altitude of 2080 feet above the sea-level. Here the telescope was mounted on the side of a hill, with its axis parallel to that of the earth, the object-glass and mirror being at the lower end. In consequence of the great focal length the necessary motion was imparted to the mirror by electro-motors, so as to be capable of control from the eye end. The arrangement worked very satisfactorily, but difficulties were encountered in other directions. It had been ascertained before starting that the mirror was not flat; how serious a matter this was was not discovered until the whole was set up, and then it was found necessary to reduce the aperture of the lens to 6 inches, thus sacrificing three-quarters of the light, and necessitating prolonged exposures varying from 80° to 600° . For two only of the plates reproduced in the atlas the full aperture of 12 inches was used with exposure 30° , and for one of the full Moon an aperture of 9 inches with exposure 15° was sufficient. The seeing, too, although good, was found to be decidedly inferior to that during the summer months, although there were fewer clouds in winter. The combination of these two causes, prolonged exposure and inferior seeing, has doubtless rendered the definition less perfect than it otherwise would have been; and although this must necessarily give rise to some disappointment, all the more credit must be awarded to Prof. W. H. Pickering and his assistant, Mr. E. R. Cram, for the skill and perseverance with which the difficulties were surmounted.

For arrangement in the atlas the Moon's visible surface has been divided into eight zones by planes perpendicular to the equator and equidistant from one another; these, with the equator itself, divide the disc into 16 parts, and each part is represented under five different illuminations, designated respectively "sun-rise," "morning," "noon," "evening," and "sunset"; the plates

are reproduced by the "half-tone" process, and are the exact size of the original negatives, on which 5" of arc corresponds to 1 mm., the diameter of the disc varying from 14 to 16 inches. The scale is smaller than that of other photographic atlases, and some detail is sacrificed in consequence, but the atlas is rendered more handy for use, and the texture of the grain does not appear. The plates, which are in strips some 4 inches wide by 9 inches long, have at first sight an unwonted, curtailed appearance; but this is more than compensated by the convenience of the arrangement, which renders it easy to turn at once to the plate in which any desired formation is represented under any one of the five illuminations, or to trace its changes in appearance during the progress of the lunar day. The atlas is not in itself likely to add to our knowledge of the Moon; the plates do not show as much detail as those taken under corresponding illumination in the Paris atlas, where such are to be found, or as the exquisite photographs obtained at Yerkes, and yet the whole is so complete that it marks one of the greatest advances in lunar cartography since the appearance of Schmidt's map in 1878. In it it is possible for the intending observer to familiarize himself with the general outlines of any region he may desire to study, and its apparent changes under different illuminations, and so enable himself to make a better use of the short periods available for that real work at the telescope on which progress in this branch of selenography must depend. Even the best photographs at present obtained do not show anything like the amount of detail that may be seen during favourable instants with a 5- or 6-inch telescope, but it is only the observer who has already made himself familiar with a particular region who is able to take advantage of these instants, or who can tell which out of all the multiplicity of detail presented to him is hitherto unrecorded, or to fix its position by referring it to that of other detail already known. It is in this way that the atlas may prove of real use to the serious selenographer, whilst it will be of even greater value to the larger number of those who wish to obtain a general acquaintance with the principal features of our satellite.

The atlas has been issued simultaneously in two publications—in vol. li. of the Harvard 'Annals,' and in Prof. W. H. Pickering's work on 'The Moon'; the plates in the two volumes are identical except for the lettering in the second, and except also that in it plate 3 B has suffered inversion under the hands of the printer.

In the latter volume Prof. W. H. Pickering gives a popular account chiefly of his own work on the Moon, much of which has been already published in the Harvard 'Annals' and elsewhere. Some notice is taken of previous work, but the title of the book as "a summary of the existing knowledge of our satellite" is misleading; and although this is set right in the first paragraph of the preface, where it is explained that "it is intended to give an account of some of the more recent advances in our knowledge of the Moon, leaving to the text-books a statement of

the information that was earlier acquired," still it is a pity that the titlepage was not corrected accordingly. It is a pity, too, that Prof. Pickering has adopted the term co-longitude to denote longitude reckoned from the central meridian eastwards through 360° ; the connotation of the term has already been definitely established as denoting 90° (or 450°) *minus* longitude, a concept which would have served his purpose of recording the position of the sunrise terminator at least as well as the one adopted, which is 90° less (or 270° more) than the recognized value. Such a use of already accepted terms can only lead to confusion.

Whatever may be thought of Prof. Pickering's well-known opinions as to the existence of ice and of vegetation upon the Moon, or as to the frequency of volcanic eruptions, there can be no doubt that he has, at Arequipa and at Mandeville, had opportunities for observation such as have been enjoyed by no other selenographer, and that his explanations are as consistent as any others that have been proposed to account for the appearances; if, in the opinion of some who have not had his advantages, his conclusions are not yet proved, he has at all events gone a long way towards rendering them probable. It is doubtful, however, whether his argument is much advanced by the printing of such photographs as one which appears on plate C of 'The Moon,' where the maria are absolutely invisible, and the detail is reduced to a series of bright patches which are stated to be ice. He himself points out that appearances of this sort depend largely on the exposure and development of a plate, and in another part of the book he calls attention to an instance in which an apparent change in Plinius "is probably not due to the melting of the snow, but is merely an effect of photographic contrast." The critical reader will ask whether much of the ice may not be also an effect of photographic contrast. Prof. Pickering, in commenting on this same photograph, calls attention to the fact that the entire limb of the Moon is dark except for two bright polar spots. Others have found a marked increase of brilliancy as the limb is approached, and MM. Lœwy and Puiseux, who have devoted several pages to a discussion of the matter, find the increase to be independent of the phase and almost of the latitude. This brightening of the limb is observable also on the Lick photographs and on some of those in Prof. Pickering's own atlas.

With regard to Prof. Pickering's telescopic observations, there can be no doubt that the method he has adopted of devoting his attention exclusively to a few very small selected regions is the right one for those who wish to make any real advance in our knowledge of the Moon.

In constructing the index-maps to his atlas Prof. Pickering has adopted the positions found by Franz for 150 formations, and with these as data has drawn a series of meridians and parallels upon a photograph of the full Moon obtained on 1901 August 29. Franz's positions are without doubt the most accurate yet

published, and when Prof. Pickering states that 40 per cent. of the positions he finds for other formations differ by more than 1° from those given by Neison, his belief that it is the latter that are at fault would seem to be well founded.

The last chapter in the volume of the Harvard 'Annals' is devoted to a discussion of the effect which the altitude of a mountain must have upon its apparent selenographical position when measured under different librations, and whether our present measures are sufficiently accurate to deduce the altitude of the mountain above the mean spherical surface of the Moon from this apparent change of position. No mention is made of any measures having been made at Harvard, and the discussion is based on measures made by Franz and published in '*Breslau Mittheilungen*,' Bd. i. The task of the reader would have been much lightened had a little more precision been given to the language. Thus, when a symbol is taken to represent "the altitude of a station expressed in minutes of arc," it might have been added that this arc was that of a great circle of the Moon. It is not usual to express the height of a mountain in this way, and the reader can only tell what is meant when he sees what is made of it. Or again, when we are told that the accuracy with which we may determine this altitude "depends directly" on the magnitude of another quantity, and that therefore we must weight the different values of the altitude by multiplying them by this quantity, the proposed weighting is approximately correct, but the reader has to discover that the way in which the one "depends upon" the other is such as to justify it. But these criticisms do not affect the real argument, which is directed to show that the altitude of a station may be determined from the discordances of Franz's five measures with sufficient accuracy to render it worth while to apply a correction for altitude to the resulting mean, and Dr. Franz is criticized for not having done so.

Twenty points only are singled out for discussion: two of these have to be rejected because the displacements in longitude and in latitude respectively give altitudes differing by some 7 km.; several other considerable discordances remain both between the pair of values obtained at Harvard and also between the mean of these and the value obtained by Franz. One other may be noted. The lowest altitude found at Harvard is that of Hypatia B, which is placed 2.2 km. lower than Linné, and therefore 5.4 km. below the assumed mean surface. Now on turning to Franz's contour-map in Königsberg Beob. Bd. xxxviii., it is seen that Hypatia B is in the middle of a region which is coloured as being between 1.2 and 2.4 km. above the mean surface. The divergence therefore amounts to not less than 6 or 7 km., although it is true that Franz has not directly determined the altitude of this particular point.

It appears to the present writer that the displacement to be measured is so nearly of the same order as the errors of observation

that no determination of any particular altitude can be made without a far more extensive set of measures than any that has yet been published, and that the utmost we are entitled to conclude is the general elevation or depression of a region from measures of a number of craters within it. This is practically what Franz has done, and the want of confidence he felt in his own measures, considered individually, appears from the fact that when he had made only one determination of an altitude he divided it by 3 before plotting it on his chart, when he had made two determinations he entered two-thirds of the mean of the two, and there are only five points for which he made a greater number of determinations; it seems to have been the discordances exhibited by the separate values found for these five that induced him to adopt his somewhat unusual expedient for smoothing the curves. But if this is so, we may echo all the more heartily the demand with which Prof. Pickering concludes for more measures made on suitable combinations of photographs taken under very different librations; it is in this way only that we can hope to produce a really satisfactory contour-map of the Moon.

S. A. SAUNDER.

Eros and the Solar Parallax.

THE singular kindness of M. Lœwy's reply in the December number of *The Observatory* to my remarks of the previous September makes it incumbent on me to examine his methods again in the light of his latest exposition of them, to see if happily I may find a way to follow him without sacrificing the principles which seem to me to be essential. There is, however, to some extent a fundamental difference between us. M. Lœwy is the leader of the French school of photographic methods; I am a follower of the English school; and between the two there is a radical difference of view, not unlike a difference of political faith, which is hardly a subject for argument.

The French school regard measurement of a photograph as a means of obtaining, by the aid of the "étoiles de repère," *absolute* right ascensions and declinations. The English school regard measurement of a photograph as a means of obtaining the *relative* positions of objects on the plate, which may, for nearly all purposes, be conveniently expressed in the rectangular coordinates wherein the measures are made. Believing that the accuracy of the relative places *inter se* is much greater than the accuracy with which absolute positions can be obtained from meridian observations, they avoid as far as possible the use of meridian places at all.

The difference between the two policies is clearly exhibited in the methods of determining the solar parallax adopted by M. Lœwy and by myself. M. Lœwy's plan is to allow each man to

determine in any way that he thinks fit the absolute places of the repère stars from the material published in the circulars, to determine then from each plate the absolute place of the planet, compare it with the ephemeris, and use the difference between the tabular and observed places as the numerical term in an equation of condition. He thereby includes in his equations of condition not only the error of observation, but also the mean error in the adopted absolute places of the repère stars on the plate, which will differ from one plate to another, and from one observatory to another. When it happens, as I have shown that it does in the case of the Paris and Bordeaux plates, that the assumed absolute places are systematically different, the systematic error goes into the equations of condition, and forbids the combination of equations from different observatories, except after a rediscussion of the adopted places of the stars.

The method that I have used is different. My first care has been to establish, from sets of overlapping plates, a system of standard places of a list of stars selected to serve as fundamental in the reduction of the plates—about ten of them are measured on every plate. My standard system is very approximately centered and orientated, but its absolute fit upon the sky is, for parallax purposes, of secondary importance. What is really essential is that the system runs smoothly without discontinuity from star to star, so that when any measure of the planet is reduced by the well-known linear formulæ to this standard system, I am well assured that there is no systematic difference between this and any other measures of the planet reduced to the same system.

Now it is useless to criticize the methods of the French school from the standpoint of the English. But there are special reasons why we should be particularly anxious that the published results of the four French observatories shall be fully and completely available to everyone, no matter what his photographic faith may be. These four observatories are, I believe, the only observatories which are carrying out to the full the very arduous programme of measurement proposed by M. Loewy, which requires that something like sixty stars on the average are measured on each plate; they are the only observatories which have completed any large part of their work, and, I fear, the only observatories which will be able to publish within any reasonable time on the lines laid down by their leader. For the greater part of the co-operating observatories it is entirely out of the question to adopt the programme of measuring sixty stars per exposure, and reducing the measures to right ascension and declination. The utmost that can be demanded of them is the minimum, viz., measurement and publication of some eight or ten comparison stars on each exposure. They need a guide to the choice of stars to measure, and they need some uniform system of places of the comparison stars to which their measures may be reduced. It is natural to hope that the basis for

all this work may be found in the results which are in course of publication with such excellent promptitude in the Paris circulars. That is why the form of publication of the French work is a matter of especial concern to everyone who is interested in this new determination of the solar parallax. Perhaps M. Lœwy will pardon me if I point out one or two considerations which suggest that the adopted form is, in some respects, not generally convenient.

The published results of the Paris and Bordeaux observatories are arranged in four tables.

Table I. gives the mean positions (R.A. and Decl.) of the "étoiles de repère" deduced from the whole series of plates, the places of these stars assumed at the beginning as the result of the meridian observations, and the number of plates on which each is found. It is not possible to discover from this table what repère stars were measured on any particular plate.

Table II. gives the R.A. and Decl. deduced separately from each exposure of all stars in a square of 20' about the planet. Since there is no indication of what repère stars belong to each plate, it is not possible to correct these results for the effect of any revision of the repère places.

Table III. collects for each comparison star, that is for each star found in the list of stars which had been used for reference in filar micrometer observations, the R.A. and Decl. deduced from the various plates on which the star occurs. Some of these places, being in the 20' square, are to be found in Table II., the majority are not. There is no other indication of the plate from which any result is found, and therefore no possibility of allowing for revision of repère places.

Table IV. gives the apparent places of the planet found from each exposure.

Now it seems to me that the loss of all plate-identity in this scheme of publication of repère and comparison stars is a misfortune. It leaves to the builders upon this foundation no choice but to accept the published places as final, without the possibility of examining any points of doubt or interest which may arise. We know that the Bordeaux results differ systematically by an appreciable quantity from the Paris results. But I am indebted to M. Lœwy for the information that the assumed repère places of Paris, Algiers, and Toulouse will be identical. If we want to make a homogeneous system we shall have to begin by discarding the Bordeaux results, because we have no means of reconciling them with the others. If any further doubt as to homogeneity should arise, we are at the end of our resources. I do not think that this can be considered a satisfactory position. It would be ameliorated if plate-identity had been preserved, but the labour of collecting and revising the reduction of the scattered results would be very great. And it seems to me that a very excellent principle adopted in the publication of the *Astrographic Catalogue* is well worth adoption here. The makers of that catalogue are given the widest

liberty of doing what seems best to them, *provided that they publish the original measures of every plate in rectangular coordinates.* Where there is any question of enquiry or revision it is essential to have access to the original measures, untouched by any reduction whatever. This proposition I can state with the greater confidence, in that I am myself occupied with an investigation of the parallax resulting from the combined measures made at some eight or nine observatories on the plates of a number of days in November 1900; for this purpose M. Lœwy and M. Trepied have, with others, very kindly sent me a copy of their original measures; in the discussion of several interesting difficulties that arose, these measures in this form were exactly what was wanted, and no other form would have done a tenth as well.

I would therefore, with all deference, urge upon M. Lœwy that, in view of the high importance of the French results, it will be a misfortune if he is not able to accept the principle that, whatever may be done afterwards, it is before all things advisable to publish the original rectangular coordinates of stars and planet measured on each exposure.

M. Lœwy remarks with perfect truth that I consider this matter from only one point of view, the utilization of the observations solely for the determination of the solar parallax, whereas the various observations accumulated in the Eros campaign have an intrinsic value of their own, apart from the question of the parallax; wherefore it would have shown a want of foresight to leave these results inaccessible rather than incur certain supplementary calculations. To this I would reply, in the first place by repeating that most observatories have not the resources to carry through more than a minimum programme; in the second, that the determination at some future time of the proper motions of the belt of stars along the track of Eros is amply secured already by the labours of the French observatories; and thirdly, that in fact the measured rectangular coordinates are all that is wanted for this purpose also.

I have left myself brief space to refer to some other points which are raised by M. Lœwy's reply. My conclusion that his efforts had failed to provide the places of nearly half the stars which had been used in filar micrometer comparisons was based, to some extent, upon a misconception of the meaning of the Paris photographic magnitudes. M. Lœwy explains that the Paris magnitudes are based upon the absolute intensity of the images on the plate with a given exposure, and that the bad weather prevalent in November 1900 made it impossible to get the fainter stars, while it dimmed the brighter. It is hoped that all the stars listed will be found upon the plates taken under better conditions; but what will happen to those observers who failed to send in the lists of the stars they had used is only too clear.

With regard to my remark that "eventually someone will have to take in hand the formation of a definitive system of comparison stars

(including repère stars)," it was open to the retort that absolutely superior wisdom could not be granted *à priori* to a single scientist. If I may withdraw the suggestion and substitute for it the proposition that *any* system of repère stars, based upon *any* reasonable method of weighting individual meridian results, would have been quite satisfactory had everyone agreed to adopt it, I can add now that the plain course for the future is for everyone to adopt the system agreed upon at Paris, Algiers, and Toulouse.

ARTHUR R. HINKS.

Rainfall and Sunshine in 1903.

THE following particulars relating to results of the observations of rainfall and sunshine made at Greenwich in 1903 may be found useful in supplementing the details given in the weather notes which have already appeared in the *Observatory* in the course of last year (see vol. xxvi.). The rainfall for the year amounted to 35·54 inches, and exceeded the average for the preceding 62 years by 11·53 inches. There were 179 rainy days, exceeding the average number by 21, and the mean fall per rainy day amounted to 0·2 inch. In the three summer months (June to August) the aggregate rainfall amounted to 16·16 inches, an amount exceeding the record for any three consecutive months in all preceding years of observation, the nearest approach to this value having been 13·84 inches in the summer of 1888.

TABLE I.—Monthly Rainfalls recorded at the Royal Observatory, Greenwich, in the Year 1903.

Month.	Rainfall.	Average for preceding 62 years, 1841–1902.	Departure from average.	Number of rainy days.	Greatest daily fall.
	in.	in.	in.		in.
January	2·13	1·88	+ 0·25	18	0·59
February	1·37	1·48	– 0·11	10	0·39
March	2·20	1·48	+ 0·72	18	0·38
April	1·85	1·57	+ 0·28	11	0·53
May	1·95	1·92	+ 0·03	15	0·34
June	6·07	1·95	+ 4·12	10	1·32
July	5·27	2·38	+ 2·89	13	2·47
August ..	4·82	2·32	+ 2·50	17	1·15
September.....	2·24	2·16	+ 0·08	15	0·77
October	4·44	2·80	+ 1·64	24	0·72
November	1·93	2·22	– 0·29	16	0·68
December	1·27	1·85	– 0·58	12	0·58
For the Year .	35·54	24·01	+ 11·53	179	—

The sunshine in the year amounted to 1445 hours out of a possible amount of 4458 hours, giving a percentage of the possible duration of 32, and exceeding the average by 127 hours. There were only 73 *absolutely* sunless days during the year, being 14 days less than the average for the preceding 16 years. The sunniest days were May 23 and June 4, with a record on each day of 13·9 hours, but the highest percentage of sunshine on any day was attained on August 7, amounting to 88 per cent. of the possible amount.

TABLE II.—Sunshine recorded at the Royal Observatory, Greenwich, in the Year 1903.

Month.	Sunshine.	Departure from average.	Percentage of possible amount.	Greatest daily record.	Number of sunless days.
	h	h	per cent.	h	
January	57·2	+ 27·9	22	5·8	12
February	73·3	+ 20·5	27	8·7	10
March	132·6	+ 42·7	36	9·5	3
April	128·9	— 8·5	31	9·3	4
May	182·9	+ 1·1	38	13·9	1
June	187·9	+ 5·0	38	13·9	6
July	184·1	— 1·3	37	13·0	2
August	188·0	+ 15·9	42	13·3	0
September.....	169·3	+ 34·6	45	11·2	2
October	84·3	— 0·3	25	7·7	6
November	35·4	— 7·2	13	4·9	11
December	21·4	— 3·6	9	5·2	16
For the Year .	1445·3	+ 126·8	32	...	73

W. C. N.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Rotation of Saturn.

GENTLEMEN,—

In my letter to the *Observatory* of November 1, 1899 (printed in vol. xxii. p. 440), I stated that Sir John Herschel gave, not only in his "Treatise on Astronomy" for Lardner's 'Cabinet Cyclopædia,' but in the successive editions of the 'Outlines' which grew out of that smaller work, the value of the rotation of Saturn erroneously quoted by Bailly as that of Sir William, *i. e.* 10^h 29^m instead of 10^h 16^m. I have lately noticed that, by an extraordinary inadvertence, Sir John, in the fifth edition,

substituted for this the value $9^h 57^m 1^{\cdot}91$, attributed to Airy. This remained until the last (tenth) edition, which appeared in 1869. It is scarcely necessary to mention that the value in question is Airy's value of the sidereal period ($= 9^h 55^m 24^{\cdot}10$ in solar time) of the rotation of *Jupiter**, not Saturn, as given in vol. iii. p. 22 of the 'Monthly Notices of the R. A. S.' The only known value determined by Sir W. Herschel for the rotation of Saturn is $10^h 16^m 1^{\cdot}44$ (approximately $10^h 16^m$, as stated above), of which he confidently expresses the opinion that it is not more than two minutes in error. For the *equatoreal* rotation subsequent measures have confirmed this estimate; but now we know that the apparent rotation, like that of Jupiter and even in a greater degree, increases as the observations are farther from the equator, one cannot help sometimes doubting whether Sir William may, at some time, have found a longer duration of the rotation, and that this remained in Sir John's memory. Probably the moral is that in quoting numerical statements it is always desirable to give the authority.

Yours faithfully,

Blackheath, 1904, Jan. 4.

W. T. LYNN.

Astronomy in the Book of Joshua.

GENTLEMEN,—

I have very little faith in early astronomical observations of any kind, and that recorded in the Book of Joshua is not an exception. In the first place, what does the Hebrew text mean? In the margin of the Revised Bible we have "be silent" instead of "stand still," and some Hebrew scholars interpret "be silent" as "be inactive," *i. e.* withdraw thy light. If what followed was really (as our version implies) a hail-storm of unexampled violence we can hardly believe that the Sun continued to shine steadily on Gibeon during its continuance.

On the assumption that the "silence" of the Sun meant his invisibility, I would suggest that he may have been obscured by a dense cloud of meteorites, part of which fell with such disastrous effect on the Canaanites in their flight. This hypothesis is not beyond the range of possible verification. Aerolites or meteorites can be recognized by persons who have had experience in that department, and if these Canaanites were slain by a shower of meteoric stones, we know (approximately) the locality in which to look for them. I am not aware whether such a search has been made. (Dr. Bonutz tabulates this as an early aerolite fall in his recent Catalogue.)

Truly yours,

W. H. S. MONCK.

P.S.—Perhaps the meaning may be, not that the Sun was seen in the direction of Gibeon throughout the day, but that it continued to shine upon Gibeon (*i. e.* on the city wall) throughout.

* Airy made no determination of the rotation of Saturn.

This would occur if the battle commenced on the east and swept round the south, finishing on the west of Gibeon. The wall would thus never be in the shadow.

[NOTE.—We have received several other letters about Mr. Maunder's article, which we must ask the writers to excuse us for not publishing, though they all treat the subject in an interesting manner. A view expressed by more than one of them is that the story told in the Old Testament is poetry, that the word "Moon" is introduced solely as antithesis to the "Sun," and therefore the data of Mr. Maunder's computation (which he treated purely as an astronomical problem) have no foundation in fact. It is also pointed out that the result deduced, viz., that the time of the battle was mid-day, is not in accordance with that found by others, some of whom have inferred that it was just before dawn. We hope that no one else will trouble to write letters to us for publication on this subject.—Eds.]

NOTES.

COMET NOTES.—*Ast. Nach.* 3924 contains observations of the brightness of Borrelly's Comet 1903 IV. made at Strassburg by Hans Rosenberg and C. W. Wirtz. The nucleus seems to have been of about the 6th magnitude from July 12 to August 10, while the combined light of the object rose to a maximum of 3·2 mag. between July 18 and 25. On August 13 the comet was viewed from an altitude of 3000 feet, and a Zeiss binocular showed a tail 7° in length.

A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered at Heidelberg by Dugan:—

Planet.	Date.	Mag.	Planet.	Date.	Mag.
MY.....	1904 Jan. 10	12·5	NA	1904 Jan. 10	11·3
MZ.....	„	10·5	NB	„	11·2

MY was at first supposed to be identical with MP, but this is disproved. MZ is very probably identical with 409 Aspasia, and either NA or NB is possibly identical with 505. A. C. D. C.

·PHOTOGRAPHIC REGISTRATION OF TRANSITS.—Sir Howard Grubb explained at the Royal Dublin Society on November 17 last a method that he had devised for registering transits photographically. The principle adopted is that in which the plate moves at the same rate as the star, so that the star appears as a round image, and the fixed reticule is imprinted on the moving plate at

each second. One difficulty hitherto met with in carrying out this method is in devising a means for varying the rate of motion of the plate for stars of different declination. This is effected by supporting the plate on a circular metal arc whose centre is at the centre of the horizontal axis of the transit instrument. The transit instrument is of the ordinary form as to axis and bearings, but the object-glass is at the centre of the axis, and the plate, which is always in the focus of the object-glass, is carried by a polar axis, quite separate from the transit-circle, and turned at the rate of diurnal motion as an equatorial, so that at all declinations the image of a star should be perfectly round. The diaphragm is mounted at one end of the axis, and by optical means an image of the diaphragm will be formed in the same plane as the star images, and an instantaneous shutter will give these images every second or alternate second. So far as this part of the description goes the apparatus seems workable, but we do not yet know how the adjustments or errors of the diaphragm, corresponding to the ordinary errors of a transit, are to be effected or determined.

THE INTENSITY OF THE LIGHT OF THE SUN AND OF THE STARS.—

The results of some interesting experiments on this subject, made by M. Fabry, have lately been published in the *Comptes Rendus* (1903, December 7 and 28).

The method of measuring the intensity of sunlight consisted in passing a beam of light through a lens on to one face of the screen of a photometer, and the light from an electric lamp of known intensity, properly screened so that the lights were of the same tint, on to the other face, and then equalizing the lights by moving the lens. The experiments showed the result that the Sun in the zenith at its mean distance gives 100,000 times as much light as a decimal candle at the distance of one metre.

With a very similar apparatus the light of Vega was measured, and it was found that this star produces the same effect as a decimal candle at a distance of 780 metres, or, what is the same thing, it is the equivalent of 1·7 millionths of a candle-power at the distance of a metre.

Assuming the ordinary law of photometric magnitudes, and that the magnitude of Vega is 0·2, the formula

$$m = -14\cdot2 - 2\cdot5 \log E,$$

can be deduced, connecting m , the number denoting the magnitude of a star, and E , its lighting-effect, in units of a candle-metre. Comparing with the previous determination, it appears that the light we receive from the Sun is sixty-thousand million times as great as the light we receive from Vega, so that our luminary is to us a star of magnitude $-26\cdot7$. This result is in accordance

with that of Zollner, but other investigators have found a smaller figure for the intensity of sunlight.

RETIREMENT OF MR. W. C. NASH.—At the beginning of the current year, in accordance with the rules of the Admiralty Service, Mr. Nash resigned his post at the Royal Observatory, Greenwich, after a continuous service of 48 years. On joining the Observatory staff in 1856, he was attached to the Magnetical and Meteorological Department, then under the direction of Mr. Glaisher, with whom he was associated in much of the extraneous work undertaken by that gentleman, especially the scientific balloon-ascents, in which Mr. Nash himself took part on two occasions, in 1862 and 1869. Mr. Nash's time being almost entirely taken up by the duties of his office, he had little opportunity of taking part in the astronomical work of the Observatory, his principal work in that connection being the observation of principal meteor-showers, which was a regular charge upon the Magnetical and Meteorological Department, and the making of a few of the spectroscopic observations with the Merz Equatorial in the years 1883 and 1884. On the retirement of Mr. Ellis in 1894, he was chosen to succeed him as Superintendent of the Department, which office he has ably filled for the past ten years.

WE much regret to hear of the death, which took place on January 19, of Dr. W. Francis, until lately senior member of the firm which publishes this Magazine, and for many years joint Editor, with Lord Kelvin and Dr. J. Joly, of the 'Philosophical Magazine.' The deceased gentleman, who was a Fellow of the Royal Astronomical, Linnean, Geological, and Chemical Societies was within one month of being 87 years of age when he died.

A MEETING of gentlemen interested in astronomy was held at Edinburgh on January 9 to make arrangements for resuscitating the Astronomical Institution, originally founded in 1812. It is announced that office-holders were elected, and the scheme made considerable progress.

THE evening lecture at the Royal Institution on Friday, January 22, was delivered by the Rev. W. Sidgreaves, S.J., who took for his subject "Spectroscopic Studies of Astrophysical Problems at Stonyhurst College Observatory."

SIGNOR BOCCARDI, late Assistant in the Observatory of Catania, has been appointed Professor of Astronomy and Director of the Observatory of the University of Turin.

ATTENTION may be called to an announcement in our advertisement pages that a very excellent equatorially-mounted refractor by Alvan Clark is for sale at a moderate price.

PROF. TURNER has been elected a Foreign Member of the Società degli Spettroscopisti d'Italiani.

ALDEBARAN will be occulted by the Moon on February 24.

THE Annual General Meeting of the Royal Astronomical Society will take place on Friday, February 12, at 5 o'clock. The next Meeting of the British Astronomical Association will be on Wednesday, February 24.

FROM AN OXFORD NOTE-BOOK.

WE shall be sorry not to have Professor Hale present at the Annual Meeting of the Royal Astronomical Society on February 12, to receive the Medal he so well deserves; but the reason which keeps him away is one which commands our respect. He was led to pay a visit to California to study the conditions for solar work, and I hear from him under date January 8:—"Since becoming acquainted with the admirable conditions for solar work on Mount Wilson, I felt that I must develop my solar investigations here, on a small scale at any rate . . . , and hope to bring out our new horizontal telescope in the spring. If I were to go (to England) I could not hope to get it set up on the mountain in time for work during the best season; as it is I shall be hard pressed to accomplish it. But I am sure you will know that if it were in any way possible, I would gladly make the long journey, not merely for my own pleasure, but as a slight evidence of my appreciation of so great an honour." So that, were it not for quite exceptional circumstances, Professor Hale would have braved the journey as readily as Professor Barnard did, though, it may be hoped, with better fortune than to be delayed by bad weather over the date of the Meeting. But as it becomes clear that we may from time to time expect such distinguished guests from over the water, ought we not to think of changing the date of our Annual Meeting to a less inclement season of the year? Our Medallists may not all have the courage and amiability of Professor Barnard.

THE many friends of Sir David Gill will be glad to learn that he proposes to sail for England on March 9, but equally sorry to hear that his health has not been good lately. We shall all hope that his European trip will completely restore him.

THE Royal Astronomical Society recently determined to publish in facsimile, as a memorial to the late Professor Adams, his very earliest (date 1843) solution of the problem of finding Neptune (see *Observatory*, vol. xxv. pp. 39-42); and the portion of the Memoirs containing this work, with an introduction by Professor

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R. A. Sampson (to which he has devoted much time and labour), is now nearly ready for issue. When telling us about this early solution, the existence of which had scarcely been realized, Professor Sampson drew attention to a human little touch:—

Fortunately this early solution was complete and intelligible, though it was most difficult to follow. For it was written in part on the backs and in unoccupied corners of sheets devoted to other work, as though his eagerness to finish would not wait while he got a few more sheets of paper. And, curiously, at the very point of writing down the planet's place, Adams, who carried through fabulous computations without error, makes a mistake and puts down the wrong place! (*Loc. cit.* p. 40.)

We may compare with this the history of the completion of Newton's calculation of the force of gravity at the Moon. At the moment I have only at hand for reference Grant's 'History of Astronomy,' where, in a footnote on p. 25, it is related that "Newton became so much agitated as soon as he began to suspect the probable result of his calculation that he was compelled to assign to a friend the task of bringing it to a conclusion." Happy are those who ever feel in the humblest degree this divine agitation!

THERE is another incident connected with the discovery of Neptune which has a close parallel in Newton's life. When the immortal 'Principia' was written and ready for publication, a hitch occurred because the Royal Society had no funds to publish it with. They had spent all their money on a book on fishes, and were even paying their officers in copies of this book instead of hard cash. Halley came to the rescue and published the 'Principia' at his own expense. The parallel may be found in the "Advertisement" to Adams's paper on Neptune in the 'Nautical Almanac' for 1851, as follows:—

This Paper was communicated by the Author to the Royal Astronomical Society, and was read to that body at their ordinary meeting on November 13, 1846. The Press of the Society being engaged on an extensive paper on the longitude of Valentia, by the Astronomer Royal, and it being deemed of national importance that Mr. Adams's Paper should be submitted to the world without loss of time, application was made to Capt. W. H. Smyth, R.N., President, and to the Rev. R. Sheepshanks, Secretary of the Society, who, with their usual promptitude and zeal, granted permission for the immediate printing and publishing of the Paper by the Nautical Almanac Office; and it is under these circumstances that the investigations of Mr. Adams first appear as an extract from the Appendix to the Nautical Almanac for 1851.

THE following is extracted from the *Daily Paper* of January 7:—

The latest form taken by the public benevolence of Mr. J. Passmore Edwards is an offer to provide for the Haggerston Public Library a marble memorial bust of Edmund Halley, the Astronomer Royal and mathematician, who was born in Haggerston in 1656. The bust is to be undertaken by Mr. Henry A. Pegram, the sculptor of the statue "Fortune" in the Royal Academy.

"ON May 25, 1903, the Director made his one-millionth photometric setting, having made 39,796 settings with the 2-inch, 643,308

with the 4-inch, and 316,896 with the 12-inch meridian photometers." Those who know anything of astronomical work will have no difficulty in assigning these words to their origin—the Report of the Harvard College Observatory. They record an achievement of which anyone might be proud if it were his only one, and when we remember that Professor Pickering has meantime been actively developing two observatories and organizing the efficient expenditure of large endowments, and that the observational work mentioned above is taken by way of relaxation from the cares of administration, we feel that the word "marvellous" may be used without overstating the case. Our American cousins have already done much to familiarize us with millions in the department of finance, where "circumstances over which we have no control" often prevent us following their lead; but this is a newer and nobler illustration of numerical magnitudes in a department where we meet on common ground. What are we going to do about it?

At the January Meeting of the Royal Astronomical Society an interesting discussion was held, as will be found reported on another page, concerning the preparation of the beautiful lantern-slides with which Mr. Ritchey has made us familiar. He has publicly stated that in preparing slides for exhibit, local development is freely used to bring out their merits plainly, though at the same time another negative is always stored which has been developed in the ordinary way. There would seem to be nothing but gain from this procedure, and yet it has been adversely criticized, probably because imperfectly understood. To the proper understanding of it Mr. Wesley made a most important contribution in the declaration that he could find nothing, after a searching scrutiny, which had been introduced into the slides by the special treatment. Time limited the discussion, and of course nothing like a vote was taken or would have been under any circumstances attempted: but speaking as one who followed the discussion closely, I think the feeling of the Meeting was clear on two points. In the first place, when the issue was clearly understood, no one was prepared to make any adverse comments, though the President made a direct enquiry; and secondly, a distinct murmur of applause greeted the suggestion that it would be convenient to have all specially-prepared slides marked in some distinctive way.

"MARS is signalling a dark star."

The world to which this news was flashed from the Central Observatory on the Himalayas had long been dull and stagnant. Almost every scientific discovery had been made thousands of years before, and the inventions for their application had been so perfected that it seemed as if no real improvement could be made in them.

These are the opening words of a thrilling story about the "End of the World" by Simon Newcomb, in *McClure's Magazine* for

May 1903, which I have only just seen. Others possibly have not even yet read of it, and may be glad to secure a copy of it, for it is impossible to do justice to the story in a note. The plot is briefly that the dark star hits the Sun and makes him blaze up so as to give the Earth a most uncomfortable time. The pictures are especially interesting, for in spite of what is said in the extract quoted about the perfection reached by all inventions, the crowd is represented pictorially watching the approaching star through telescopes of the modern 3-inch pattern mounted on tripods, and fire-engines of the present day *drawn by horses* are galloping to put out the conflagrations caused by the catastrophe. Is it permitted to draw the comforting conclusion that in at least two directions we have already reached perfection? The closing words of the story may be quoted. They are uttered by the "Professor of Physics," who, wise as Noah, had shut up his family in a safe place when the catastrophe became certain, hoping to repopulate the Earth when it was over:—

But now every such prospect dissolved away. As their strength ebbed, a holy calm spread over the souls of all. The Professor found words:—

"Such is the course of evolution. The Sun, which for millions of years gave light and heat to our system, was about to sink into exhaustion and become a cold and inert mass. Its energy could not be revived, except by such a catastrophe as has occurred. The Sun is restored to what it was before there was any Earth upon which it could shed its rays, and will in time be ready to run its course anew. In order that a race may be renewed it must die like an individual. Untold ages must once more elapse while life is reappearing on Earth and developing in higher forms. . . ."

THE following extracts from the daily press may provide some little amusement:—

MOON'S BROAD SMILE.

The Moon, for some unaccountable reason, rejoiced in a beaming smile on Saturday night. Through the thin veil of fog which hung near mother earth our heavenly nightlight had the effect of transforming the most pessimistic human mood into one of cheery optimism.

At first sight her face seemed to wear a sneer of the coldest type rather than a smile, but as one gazed at her the expression developed into a hearty grin. Indeed, a Hampstead correspondent goes so far as to suggest that in the safe seclusion of the heavens the Moon was laughing at the world, untroubled by any possibility of being thrown into the arena of a fiscal fight.

The record for rain in 1903 is cheerfully regarded by a correspondent, who remarks that according to Bishop Jewel the year 1562 was at least as damp. In the autumn the Bishop wrote to his friend Bullinger, of Zurich, a letter containing the passage:—

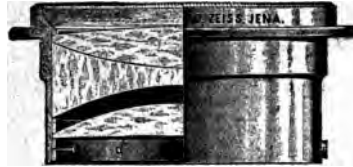
"The weather and temperature during the whole of this year have been past belief inclement. It rains unweariedly and almost without intermission, as if the sky could no longer do anything else. As a result of this plague infant prodigies are born, some without heads, others with the heads of monsters, others without limbs, others mere living skeletons."

A weather prophet has written to us confessing that his forecast was quite wrong. We have suffered many things of many weather prophets, but never before has one denounced his own infallibility.—*Daily News*, Jan. 5.

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LONDON.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

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MARCH, 1904.

No. 342.

ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 February 12.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

Mr. Whittaker read the Minutes of the last Annual General Meeting, which were confirmed.

Mr. C. Thwaites presented the Report of the Auditors, which stated that they had examined the Treasurer's accounts for the year 1903, and found and certified the same to be correct. The cash balance in hand on 1903, December 31, was £260 16s. 3d. as compared with £521 6s. on 1903, January 1. They had inspected the books, instruments, and other effects in the possession of the Society, which appeared to them to be in good condition.

Mr. D. G. Simpson asked a question relating to the capital of the Society.

Mr. Maw. The investments of the Society have been increased during the year by the purchase of £500 Lancashire and Yorkshire Railway 3 per cent. Preference Stock. With this exception, the funded property of the Society remains as in former years.

The Secretaries then presented the Report of the Council to the Society.

Mr. Whittaker read extracts from the Reports of Observatories, which included, besides the Royal and University Observatories, those belonging to the following amateur astronomers:—Mr. Espin, Dr. Isaac Roberts, Mr. Saunder, and Mr. W. E. Wilson. There was also a Report from the Rousdon Observatory (late Sir Cuthbert Peek's; *Observer*, Mr. Grover).

Mr. Dyson read extracts from the obituary notices of Fellows who had died during the past year. The list contained the names of the following Fellows:—John Henry Brown, E. T. Carter, A. A. Common, Dr. C. H. Gatty, James Glaisher, William Harnett, Dr. Ambrose Kibbler, Dr. Samuel Kinns, Thomas Mackenzie, A. B. Martin, Francis Murray Newton, William Irving Page, Francis Cranmer Penrose, Lord Pirbright, Harold Seward, Washington Teasdale, Major-General Wardell, William Livingstone Watson, and of one Associate, Prosper Henry.

Mr. Whittaker read the list of the subjects reported on in the Progress of Astronomy during 1903, with the names of the authors of the notes on these subjects:—Meteoric Astronomy, Solar Prominences, The Comets of 1903, Double Stars, Variable Stars, Solar Activity in 1903, Boss's Standard Catalogue, Auwers's Reduction of Bradley's Observations, Comets' Tails, the Rotation of Saturn, the Astrographic Catalogue, and Geodesy.

The Chairman then announced that the Gold Medal of the Society had been awarded to Prof. G. E. Hale, Director of the Yerkes Observatory, for his method of photographing the solar surface and other astronomical work. In a brief but exhaustive address, he gave to the Meeting an account of Prof. Hale's work as the grounds for the award. Whilst Prof. Turner was speaking, the Hon. J. H. Choate, the American Ambassador, entered the room, to whom the medal was handed, at the end of the speech, for transmission to Prof. Hale.

The Hon. Mr. Choate. It is only three years ago since I was last in this room to receive a medal on behalf of Prof. Pickering. Need I say what a pleasure it is for me to be here again to receive a similar medal on behalf of another American astronomer? It may not lessen the value of this honour if I take it as a symbol of our cousinship. I consider it a great honour to be requested by Prof. Hale, whose engagements will not permit him to be present to represent him here, and to be invited by the Society to receive, on his behalf, the crowning honour that the world has for an astronomer. Our astronomers in America have rare facilities—fortune smiles on them: with three hundred fine nights out of the three hundred and sixty-five, and supported by public and private munificence, every possibility is theirs. Prof. Hale has written to me that the award of this medal will be a stimulant to him for further exertion, and to give stimulus and nerve to such a man is the highest purpose of such an award.

General Waterhouse proposed, *Mr. McClean* seconded, that the Report of the Council be received and adopted, and that it be printed and circulated in the usual manner, together with the President's Address and the Report of the Auditors.

The President appointed Mr. R. Inwards, Mr. W. T. Lynn, and Mr. H. P. Hollis, as Scrutineers of the Ballot for the election of Officers and Council for the ensuing year. The Ballot was then taken, and the following were declared duly elected:—

President: Prof. H. H. Turner. *Vice-Presidents*: W. H. M. Christie (Astronomer Royal), J. W. L. Glaisher, Major P. A. MacMahon, E. J. Spitta. *Treasurer*: W. H. Maw. *Secretaries*: F. W. Dyson, E. T. Whittaker. *Foreign Secretary*: Sir William Huggins. *Council*: Sir Wm. Abney, Sir R. S. Ball, Rev. A. I. Cortie, Alfred Fowler, Major E. H. Hills, A. R. Hinks, E. B. Knobel, T. Lewis, Frank McClean, H. F. Newall, Capt. W. Noble, A. A. Rambaut.

Mr. C. Thwaites proposed, *Mr. Bryan Cookson* seconded, that the thanks of the Meeting be given to the retiring Members of the Council.

A vote of thanks, proposed by *Mr. McClean* and seconded by *Mr. W. W. Bryant*, was accorded to the Auditors of the Treasurer's Accounts and to the Scrutineers of the Ballot.

The following gentlemen were elected Fellows of the Society:—

J. D. Bharda, New High School, Bombay, India.

Tyson Crawford, 35 Ludgate Hill, E.C.

E. V. Heward, Hereward Cottage, Westgate-on-Sea.

Capt. E. J. Sharpe, Newport, Monmouthshire.

E. Spiegel, 120 Bishopsgate Street, E.C.

P. E. Vizard, Belsize Lodge, Belsize Lane, Hampstead, N.W.

The following Candidates were proposed for election as Fellows of the Society:—

Prof. J. B. Dale, M.A., New Malden, Surrey (proposed by Major MacMahon).

Rev. W. C. Eppstein, M.A., School House, Reading (proposed by Rev. J. M. Bacon).

Walter Nuttall, B.A., Hallfold, Whitworth, Rochdale (proposed by Rev. E. Wainwright).

W. E. Rolston, Solar Physics Observatory, South Kensington, S.W. (proposed by W. J. S. Lockyer).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1904 January 27.

President: S. A. SAUNDER, M.A., F.R.A.S., in the Chair.

Secretaries: A. C. D. CROMMELIN, B.A., F.R.A.S.
J. G. PETRIE, F.R.A.S.

Among the presents announced, the President called particular attention to the Harvard College Observatory Annals, vol. 51, containing the first complete photographic atlas of the Moon which had ever been published.

The names of five candidates for Membership were read, and passed for suspension, and the election by the Council of four new Members was confirmed.

Mr. G. J. Newbegin read a note on "Solar Prominences, 1903."

Mr. C. Thwaites asked if *Mr. Newbegin* had noticed any relationship between the prominences and the spots on the Sun as they neared the edge of the disc.

Mr. Newbegin did not think he ought to answer the question in the affirmative, because he studied the prominences mostly, and the records of his observations went to *Father Cortie*, who had a far more elaborate record of sun-spots than he (the speaker) had. He believed *Father Cortie* was looking into the point.

Mr. E. Holmes asked *Mr. Newbegin* what he considered the minimum of apparatus for seeing the prominences.

Mr. Newbegin said that it was a rather difficult question to answer. He himself used an instrument of extra large size, but he saw no reason why smaller telescopes and spectroscopes should not do prominence work.

In response to a question from the President, *Mr. E. Maunder* said that working with a narrow slit, an ordinary prism-spectroscope, costing £3 to £5, might be made sufficient for the purpose.

Mr. Thwaites thought that *Mr. Evershed* habitually used a 3-in. or 3½-in. refractor for his prominence work.

Mr. Holmes read a paper on "The Aspects of the Nebular Hypothesis."

Mr. Holmes began by suggesting that in the present state of the question the title "nebulous hypothesis" would be a more fitting one for the theory, and quoted a number of contradictory opinions on Laplace's hypothesis from leading authorities. Laplace assumed an enormously high initial temperature, while a transcendently hot nebula was not easily conceivable, but this had been got over by the demonstration that a diffuse mass, although actually cold, might contain vast stores of potential heat. We made, in fact, our nebula hot or cold as suited our immediate purpose, which was convenient. The nebula, which is cold, contracts because it cools, and gets hotter because it contracts. But bodies expanded when they got hotter instead of contracting. Again, the equilibrium of gaseous globes defied treatment, and there was no possibility of dealing mathematically with bodies in any other than the liquid state. No doubt a body of gaseous matter, forcibly compressed, got hotter; but we were taught that an unconfined mass of gas tended to expand indefinitely and without limits, and it was hard to see why it should at the same time be able to contract. As the primitive nebula had to condense into a solar system, it must have contained the materials of that system, and, to take only one element as an instance, it must have contained iron, and as it was all nebulous the iron must have been nebulous. Cold iron vapour was difficult to imagine. Could a gas, subject to no attraction but its own mass, exert any pressure upon

its internal parts at all? Could a gas, without solid or liquid core, hold together at all? There seemed to be no evidence whatever that any existing nebula contained such material as could form a solar system. They were of almost incredibly tenuous structure and very slight luminosity, which was confined to four lines of definite refrangibility. None of these lines could be certainly identified with any terrestrial substance. Four lines could not at most give evidence of more than four elements, so that the spectroscopic evidence, so far as it went, was dead against the presence of our earthly elements. The nebular hypothesis was a grand conception and had been of much service, but if it was to render service in the future, it must be cleared of its contradictions and become a scheme consistent in itself, both as regards facts and arguments. Lane's law, which appeared to be the text for many reasonings about it, stated that when a spherical mass of incandescent gas contracted through the loss of its heat by radiation into space, its temperature continually became higher as long as the gaseous condition was retained, which reminded one of the celebrated definition of a lobster as a red fish that walked backwards. Nebulæ were not spherical, they were not incandescent, and they were not intensely hot. They were justified therefore in rejecting Prof. Newcomb's statement that the spectroscopic revelation of the gaseous nature of the true nebulæ confirmed us in the opinion that these masses would all at some time condense into stars or clusters of stars, and accepting instead his hint that they were formed of matter different from any found on the surface of the Earth. Since the nebular hypothesis, although so largely accepted by implication, was also so largely discredited, and since evidence on so many points was lacking, and on others was against it, we should at least keep an open mind to the idea that nebulæ and stars were a concurrent mode of existence instead of being stages in a process of development.

Mr. G. M. Seabroke said that the nebular hypothesis, like all wide hypotheses, was very easy of attack. *Mr. Holmes* had called in question Lane's law, but he (the speaker) had never heard of its being attacked, successfully at any rate. *Mr. Holmes* doubted the possibility of the existence of cool iron vapour, but the molecules of iron might conceivably exist widely separated, and this condition might be expressed as gaseous.

Mr. Maunder said that *Mr. Holmes* had made out something of a case for rejecting particular phases of the nebular hypothesis, but not at all a case against the positive evidence in favour of a nebular theory in general. The evidence was very strong that the solar system was the result of a growth, and when they looked from the solar system to the very much vaster sidereal system, he thought that there also they had evidence that it was a single structure—a single growth. Some few years ago he, the speaker, had come to the conclusion that nebulæ and stars were essentially one—that many classes of nebulæ, at any rate, were practically

star-clusters of a particular type. The outlines of many nebulae were in parts so sharp and well defined that it was impossible to believe that they were dealing with mere diffused gas uncontrolled by the attraction of a central body.

Mr. Chambers said the subject under discussion had always seemed to him a very difficult one because of the great scarcity of facts. Had they any evidence showing that in the nebulae and clusters any change was going on? He could not accept the evidence of drawings, however skilful. Photography, after a run of two or three centuries, might furnish some evidence of change or the contrary, but during the past thirty years or more had photography disclosed any signs of change?

Mr. H. Ellis thought that a study of Dr. Roberts's photographs partly answered Mr. Chambers's query. In the second volume of Dr. Roberts's photographs there was a table of position-angles of various stellar or star-like condensations in which differences of measurement were shown as compared with previous observations. He thought that to anyone who studied the actual photographs of nebulae, it was almost impossible to resist the feeling that nebulous matter and stars were exceedingly intimately connected.

Mr. A. C. D. Crommelin said they were much indebted to Mr. Holmes for raising the question now under discussion. Of course, it was, as Mr. Chambers had said, only a hypothesis, but he thought that there was a great deal to be said for a modification of Laplace's hypothesis. His own idea was that the Andromeda Nebula probably presented somewhat the appearance of the early stages of the solar system.

Mr. W. T. Lynn said that the nebular hypothesis of Laplace did not contemplate a rotating sphere of the size of the orbit of Neptune.

Mr. H. Ellis showed on the screen a photograph of the Orion Nebula, taken with a 20 $\frac{1}{4}$ -inch reflector, an exposure of one hour being given.

The President congratulated Mr. Ellis upon having produced such a beautiful photograph. With regard to the discussion raised upon Mr. Holmes's paper, he thought they would be right in saying that the nebular hypothesis was still only a hypothesis, in fact we were only just beginning to know anything about the genesis of worlds; but we were beginning to know, with regard to our own system, something of the way in which it had been formed. Prof. Darwin had worked out a theory which gave a very satisfactory account of the formation of our own Moon.

A paper on "The Study of the Milky Way," by *Mr. I. F. H. C. Gregg*, was read by *Mr. Crommelin*. The writer quoted from the Presidential Address to the British Association, at Cardiff in 1891, by the President of the Royal Society, that the decipherment of the Milky Way must form one of the important astronomical works of the coming, now the present, century. Almost at the close of this address he spoke of the British Astronomical Asso-

ciation formed in that same year, and remarked on its already great numerical strength. No more interesting or appropriate branch of astronomy presented itself to the members of the Association than this, which was essentially a naked-eye study. What was wanted was a Milky Way Section, the members of which would make a special study of this wonderful object.

Mr. Maunder said he certainly thought it would be a very good thing if such a Section could be started.

Mr. Crommelin thought Prof. Newcomb's recent book on 'The Stars, a Study of the Universe,' brought out very forcibly the true grandeur of the Milky Way, because he there demonstrated, perhaps for the first time, how enormously distant it was.

A paper on "Cometary Refraction," by *Mr. A. M. du C. Muller*, and a note on "The Aurora Borealis," by *Mr. A. Noel Neate*, were then read. These were followed by the exhibition on the screen of a photograph of the Sun, taken in "K-light" at the Yerkes Observatory, which was remarkable for the pronounced character and coarse structure of the mottling. Four lunar photographs, taken at Paris with the large equatorial coudé, followed. They represented whole discs under different illuminations, and exhibited a remarkable amount of fine detail.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

THE monthly Meeting of this Society was held on Wednesday, February 17, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker*, President, in the Chair.

Mr. E. Mawley presented his "Report on the Phenological Observations for 1903." He showed that, owing to the mildness of the winter and early spring, wild plants flowered in advance of their average dates until about May, after which time only backward dates were recorded. In no previous year, since the present series of reports were first instituted in 1891, have such spring migrants as the swallow, cuckoo, and nightingale been so late in reaching our shores. The yield of wheat, barley, potatoes, turnips, and swedes was somewhat under average; but all the other farm-crops yielded well, especially those of hay and beans, which were unusually abundant. On account of the wet and protracted harvest, most of the grain of the cereals was more or less discoloured, while potatoes were almost everywhere much diseased. Throughout the country this was one of the most disastrous years for fruit ever known. In fact, the only crop which gave anything like an average yield was that of strawberries.

Mr. W. H. Dines gave an account of the observations which he had made by means of kites at Crinan, off the west coast of Scotland, during last summer. These observations were carried out

by Mr. Dines under the auspices of a joint committee of the Royal Meteorological Society and of the British Association, the Government Grant Committee of the Royal Society providing funds for the hire of a vessel for the purpose. The author, after describing various improvements which he had effected in the kites, stated that the weather last summer was most unfavourable for kite-flying, as not only was there heavy rainfall, but gales were of frequent occurrence. On one occasion a thunderstorm came on suddenly when the kite was at an altitude of about 4500 feet, but, fortunately, it was got down without any mishap. The results of the observations show that, in August last, the mean temperature-gradient for the first 5000 feet was $3^{\circ}2$ per 1000 feet. This is substantially the same as that obtained during the preceding summer, although the conditions of weather were very different.

An Eclipsing Star of Long Period.

THE results lately obtained at Potsdam in connexion with the variable star ϵ Aurigæ are of very peculiar interest. Whatever the mode of interpretation eventually applied to them, it can hardly fail to introduce some degree of clarification into the turbid perplexity of thought stirred by the caprices of stellar light-change. A minimum of the object now in question was noted by Fritsch in 1821; but the fact slipped out of mind, until recalled by the recurrence of phases of instability observed in the forties by Schmidt and Heis. Then Chandler duly enrolled ϵ Aurigæ as irregularly variable from 3.0 to 4.5 magnitude, and, as at any rate a suspect, it has been, for some thirty years back, kept under surveillance in the well-policed northern sky. Miss Maury took its spectrum as typical of her Group xii., division *c* (sharp lines); but Dr. Eberhard perceived more distinctive traits, and their investigation by him and Dr. Vogel led to the discovery that the analysed rays have really a composite origin. They proceed from two stars, one resembling α Cygni, the other showing an intermediate type, like that of α Persei or γ Cygni. Their mutual revolution was ascertained in November 1902, when most of the lines were registered as double; and the conclusion was reached that ϵ Aurigæ is a spectroscopic binary, probably of unusually long period*.

Most fitly, thereupon, Dr. Ludendorff, also of Potsdam, undertook the elucidation of the star's visual fluctuations. And the startling outcome of his enquiries was to present to our contemplation a system composed of two almost equally bright bodies, revolving in $54\frac{1}{4}$ years, and undergoing mutual eclipses at intervals of half that duration. Moreover, totality lasts, in each case, no

* *Sitzungsberichte*, Berlin, Nov. 27, 1902.

less than 313, and the obscurations, in all their phases, 727 days*. The curiosity excited by such an announcement prompted the application of any available test; and one lay ready at hand. On March 31, 1902, according to Dr. Ludendorff, ϵ Aurigæ was centrally eclipsed. On that day, therefore, the stars were crossing the line of sight; their radial motion was zero. (We assume a circular orbit in the absence of any evidence of eccentricity.) But on November 22 following, the relative velocity in line of sight was estimated from Dr. Hartman's best plate at 30 to 40 (say 35) kilometres a second. This was 247 days after mid-totality, and the arc traversed in the interim, on the hypothesis of a $54\frac{1}{4}$ -year period, was $4^{\circ} 27'$. In the proportion, then, of radius to the sine of this small angle, the orbital speed of the stars exceeds their measured radial velocity; it comes out equal to 280 miles per second. Their distance apart, or the radius of their relative orbit, is, on this showing, 76,292 million miles; and the corresponding mass of the system has the astounding value (in round numbers) of 188,000 times that of the Sun.

Dr. Ludendorff's theory, as it stands, must, accordingly, be called untenable. It may, however, prove capable of modification. The research, in its twofold aspect, spectrographic and photometric, has, so far, been only initiated. It is full of promise; and its developments must prove instructive, and may perhaps prove unexpected. We await them hopefully and eagerly.

AGNES M. CLERKE.

Brilliant Meteors seen at the Leonid Epoch, 1903.

THE following are the apparent paths of some of the large meteors recorded at English stations at the middle of November 1903. No doubt the list is to some extent incomplete and imperfect, for several observers do not appear to have registered the individual meteors; others, though they recorded the brighter objects as exceeding 1st mag., failed to specify their individual brilliancy as compared with Jupiter, Venus, or the Moon. And it has been considered advisable to limit the list to such meteors as were estimated to be at least as bright as Jupiter. In several cases, however, where identical meteors were seen at several stations, observations are included which rate the brilliancy less than that of Jupiter. To have included all the meteors = 1st mag., > 1st mag., = Mars or Sirius, would have increased the list to many hundreds, if not several thousands.

It would be interesting to hear of further observations of any of the objects given in the summary. The real paths of nine of these have already been computed, and it ought to be possible to

* *Astr. Nach.* Nos. 3918-20.

Brilliant Meteors seen 1903 November 14-18.

Date.	G.M.T.	Mag.	Observed Path.		Length of Path.	Duration.	Radiant.	Notes.	Observers.
			From	To					
1903.	h m					secs.			
Nov. 14...	11 15	2	5° E. 7 Orionis.	9° N. E. Rigel.	0	...	° ?		— Mills, S. Kensington.
14...	14 12	2	58 + 20	57 + 10	9	2.5	T.	Slow.	A. S. H., Slough.
15 {	10 17	2 × 2	168 + 55	185 + 47	13	...	56 + 22	White.	A. Sullivan, Duudrum.
	10 18½	2 × 2	277 + 41	268 + 31	11	1.1		Train.	T. W. B., Sunderland.
{	13 40	3 × 2	153 + 43	185° + 28	30	...	61 + 24	Green-T.	W. F. A. Ellison, Enniscorthy.
	13 45	3 × 2	103 + 0	112 - 6	11	2.0			J. McHarg, Lisburn.
{	13 58	3 × 2	160 + 18	166 + 12	8	...	149 + 28	Stk., 1 min.	W. F. A. Ellison, Enniscorthy.
	13 59½	2 × 2	137½ + 8½	133 + 0	10	0.7		Stk., 2½ min.	C. L. Brook, Meltham.
{	14 2 +	> 2	Alt. about	6° in E.S.E.	½	...			J. McHarg, Lisburn.
	14 19	> 2	210 + 65	273 + 58	30	...	L.	Stk., 30 secs.	W. F. A. Ellison, Enniscorthy.
{	14 44	2	73 + 72½	17 + 63	22	1.2	L.	Streak.	W. F. D., Bristol.
	15 0	2	117 + 27½	111 + 28	6	...	L.		W. E. Rolston, S. Kensington.
{	15 1	< 2	150 + 27	154 + 35	8½	...	L.		"
	15 4	2	117 + 5	108½ + 1	9½	...	L.		"
{	15 15	2	51 - 15	43 - 18	8	...	L.	V. swift.	T. W. B., Sunderland.
	15 20	2	84 + 3	75 - 2	10½	...	L.		W. E. Rolston, S. Kensington.
{	15 26	2	78 + 10	70 + 8	8	...	L.		"
	15 39	2	100 + 14	90 + 9	11	...	L.		"
{	15 40	> 2	to S. of β	Ursæ Maj.	...	...	L.	Stk., 20 min.	F. H. Wright, Northampton.
	15 49	> 2	109 - 19	101 - 25	10	...	L.	Streak.	T. W. B., Sunderland.
{	15 50	V. B.	5° S. of β Ursæ	to η Ursæ Maj.	...	...	L.	Red.	F. H. Wright, Northampton.
	15 50½	Sirius.	173 + 27	197 + 19	23	...	T.		S. J. J., Bridport.
{	15 50½	> 1	α Tauri to	β Camelopardi.	40	2.5	T.		Showell & Parkinson, Greenwich.
	15 52½	> 1							

15	55±	B.	From S. ended	170	+58	...	4'0	147	-11	V. slow.	F. H. Wright, Northampton.
15	59	2	178	+19	223	+41	45			V. slow, stk.	W. F. D., Bristol.
15	59½	4	181	+27	213	+26	29				S. J. J., Bridport.
15	59½	>1	α Leonis to 5° E. θ Ursæ Maj.				40				W. Parkinson, Greenwich.
16	12	2	70	+70	19	+59	23			Streak.	W. F. D., Bristol.
16	27	2	265	+71	286½	+64	9			Streak.	A. S. H., Slough.
16	27½	Q	192	+13	195	+10	4½			White.	S. J. J., Bridport.
16	34	Q	103	+39½	74	+58	22			Streak.	A. S. H., Slough.
16	34½	Q	Nearly vertical				0'5				A. S. H., Slough.
16	38½	Q	in W. alt. 5½°.				0'5				S. J. J., Bridport.
16	40	2	164	+15	169	+12	5			Stk., 4 secs.	T. W. B., Sunderland.
16	42½	2	137½	+3	132½	-3½	8½				S. J. J., Bridport.
16	45	2	168	+16	174	+11	8				W. F. D., Bristol.
16	45	Q	92	+53	68	+52½	15			Streak.	W. F. D., Bristol.
16	45½	Q	138	+12	134	+7½	6			Stk., 1 min.	A. S. H., Slough.
17	14	2	173	+40½	83½	+8	16			Streak.	W. F. D., Bristol.
17	14½	Q	93½	+35	75	+32	15			Streak.	W. F. D., Bristol.
17	22	Q	45	+68	25	+63	10				A. S. H., Slough.
17	23	2	5-½	+28	47½	+25	10			Streak.	H. Corder, Bridgewater.
17	33	5×Q	150	+26	Nly. stationary.		0'4			5' or 6' diam.	A. S. H., Slough.
17	33	½×Q	329	+69	327	+60	9			Streak.	T. W. B., Sunderland.
17	41½	2	149	+21½	Nly. stationary.		0'4			Stk., 4 secs.	W. F. D., Bristol.
18	4	2	182	+51	195	+55	9			Streak.	A. S. H., Slough.
18	5	>2	98	+44	75	+44½	16			Streak.	A. King, Sheffield.
18	7½	Sirius.	139½	-6	138	+9	3			Streak.	" "
18	10½	>1	174	+20½	179½	+19½	5			Streak.	" "
18	34½	Q	139½	+33½	132½	+38½	7			Streak.	R. Service, Dumfries.
19	10	>Q	300	+32	307	+42	12			Train.	W. F. D., Bristol.
19	13	2	343	+36	308	+34	3'5				A. S. H., Slough.
19	13	2	47	+47	40½	+25½	6				C. L. Brook, Meltham.
12	43½	1-Q	102½	-5	115½	-11	14			Sparks.	A. S. H., Slough.
12	44½	½D	153	+42	168	+36	12			Streak.	

T. W. B., T. W. Backhouse, W. F. D., W. F. Denning, A. S. H., Prof. A. S. Herschel, S. J. J., Rev. S. J. Johnson.

obtain the necessary materials for making similar deductions with regard to a few of the others.

Real Paths of 9 Brilliant Meteors seen 1903 November 15-18.

Date.	G.M.T.	Mag.	Height at Beginning.	Height at Ending.	Length of Path.	Velocity per second.	Radiant-point.	Meteor.
	h m		miles.	miles.	miles.	miles.	α δ	
Nov. 15	10 18	$> \gamma - > \phi$	79	42	48	36	56 +22	Taurid.
	13 42	$\phi - 3 \times \phi$	72	32	47	23	61 +24	Taurid.
	13 59	$2 \times \phi - 3 \times \phi$	90	53	64	...	149 +28	Leonid.
	15 59½	$4 - > 1$	91	45	128	29	147 -11	Hydrid.
	16 45	$\gamma - \phi$	77	52	30	60	151 +25	Leonid.
	17 14½	Sirius- γ	77	52	30	45	152 +25	Leonid.
	17 22½	$\gamma - \phi$	84	63	27	67	149 +19	Leonid.
	18 7	$> 1 - \gamma$	81	60	24	44	151 +23	Leonid.
Nov. 18	12 44	$\phi - \frac{1}{2} \phi$	75	33	51	33	50 +20	Arietid.

It will be noticed by a reference to the details that several Taurid fireballs appeared during the observations. Meteoric observers have known for many years that the month of November is often signalized by magnificent meteors from the region of Taurus and Aries. These objects, by their slow graceful flights and by their long trains, as well as by their strikingly luminous aspects, have often arrested our attention while watching for the Leonids.

Bishopston, Bristol,
1902, Jan. 17.

W. F. DENNING.

Notes on some of Al-Sufi's Stars.

Most of the stars observed by Al-Sufi in the 10th century are easily identified from his descriptions, which are usually very clear and leave no doubt as to the identity of the star referred to. There are, however, some cases in which the identification is a matter of some difficulty. I have found in a few cases that Al-Sufi, although accurately describing the position of the star he observed, has apparently misidentified the star observed by Hipparchus and Ptolemy. This becomes evident if we plot Ptolemy's positions—as given by Al-Sufi—and compare the results with Al-Sufi's descriptions of the stars observed by him. This I have done in all cases where there seems to be any doubt, and I have thus arrived at some interesting results which seem to have escaped Schjellerup's notice. This examination shows clearly that

Al-Sufi did not himself measure the *positions* of the stars he observed, but merely adopted those given by Ptolemy, correcting them for the effect of precession. The value of Al-Sufi's work, however, consists in his estimates of star-magnitudes, which seem to have been most carefully made, and from this point of view his work is invaluable.

The following are some of the most remarkable cases of apparent misidentification which I have met with in Al-Sufi's work. If the reader will follow my remarks with a star map, showing stars to the 6th magnitude, before him he will easily understand the points at issue.

Gemini.—With reference to Ptolemy's 9th star of this constellation, Al-Sufi remarks: "Ptolemy says that it is found in the left side of the posterior Twin, but according to the latitude and longitude of the Catalogue (Ptolemy's) this star should be situated between the 6th and 8th (ν and A (57)), deviating a little towards the South-east. Further it is of the fifth magnitude. There is, however, no star in the place indicated. On the other hand, there is between the shoulders a star of bright fifth magnitude. Possibly this star may be the 9th; but in that case there would be an error in the Catalogue of the book, since it should be in the side, whereas it is in the shoulders, very near the 5th" (ι Geminorum). The star referred to by Al-Sufi is evidently b_2 (65) Geminorum and the neighbouring star b_1 (64), seen together as one star. At present there is no star of even the 7th magnitude near the position given by Ptolemy for his 9th star, but I fail to see why Al-Sufi should have assumed that 64 and 65 Geminorum were identical with Ptolemy's star. Ptolemy may possibly have seen a *nova* or temporary star between ν and A.

With reference to the "extern stars" of Gemini (those outside the ancient figure), there is considerable difficulty in identifying Al-Sufi's 7th star. He says: "The 7th follows the three stars [85, 81, and 74 of Flamsteed], and is of small fourth magnitude; Ptolemy says full fourth. Between it and the fourth, which is the Northern of the three, there is less than two cubits ($4^\circ 40'$) towards the South-east [this would indicate ζ Cancri]. These four stars are found between *al-gumaisá* [Procyon] and the 2nd bright star which is in the head of the posterior Twin [Pollux], nearer the bright second. The latitude of this 7th star is also in error, because it follows them in their direction towards the North [this indicates μ Cancri], while as marked on the globe it forms a triangle with the 4th and 5th (85 and 81). Ptolemy says that it is bright, but in reality it is of small third magnitude. There is not in this place a star of either fourth or fifth magnitude, if it is not a faint one of nearly the sixth magnitude." With reference to these remarks of Al-Sufi, which are very puzzling and difficult to explain, Schjellerup says: "Ce passage relatif à la 7^e étoile est tout-à-fait inintelligible; en effet, la description que nous en donne Sûfi au commencement s'applique très-bien tant pour la grandeur,

que pour la position, à ζ Cancrî, tandis que, à la fin, il parle de μ Cancrî, en changeant même la grandeur." Ptolemy's positions, given by Al-Sufi, when plotted, show the 7th star in the position described by Al-Sufi, namely forming a triangle with the 4th and 5th. This position applies fairly well to ζ Cancrî, which was probably Ptolemy's star. But it does not agree at all with Al-Sufi's 7th star, which is evidently μ Cancrî, as he says it follows the 4th, 5th, and 6th "in their direction," that is nearly in a straight line with them. If Al-Sufi's estimate of "small third magnitude" was meant for μ Cancrî, it must certainly have diminished in brightness, as it is now about $5\frac{1}{2}$. If, however, intended for ζ Cancrî, we must come to the same conclusion, as ζ is at present only slightly brighter than the 5th magnitude. Both stars have spectra of the 2nd type (F). From Al-Sufi's description of the 8th star of Cancrî it seems to be μ , and this is confirmed by Ptolemy's positions. We must therefore conclude, I think, that Ptolemy's No. 7 of the "extern" stars of Gemini was really ζ Cancrî, and that Al-Sufi was mistaken in thinking that Ptolemy's position was altogether erroneous. Ptolemy's place for ζ Cancrî is not quite correct, according to modern measures, but it is not so much in error as Al-Sufi thought. Near the position of Al-Sufi's "faint star of nearly sixth magnitude" there is one at present which was measured 6.06 at Harvard Observatory. It is DM + 19°, 1911.

Leo.—With reference to Ptolemy's 21st star of the constellation Leo, Al-Sufi remarks: "Ptolemy says it is placed with the 22nd [θ] on *al-harkafa*, the buttocks, to the South of the bright 20th stars [δ] and is of the fifth magnitude; however, between the 20th and 22nd [δ and θ] no star is visible. It is found removed by a cubit towards the North above the bright 20th star." This indicates 72 Leonis. But Al-Sufi's identification does not seem justified, as M. Flammarion points out in his interesting and valuable work '*Les Étoiles*' (p. 351). Flammarion identifies Ptolemy's star with F 71. But I cannot agree with this identification either. 71, as shown by Flammarion, lies to the east of a line joining δ and θ , but on plotting Ptolemy's positions, as given by Al-Sufi, I find that Ptolemy's 21st star lies to the west of the line joining δ and θ . I find that the nearest star in the Harvard *Durchmusterung* to the position of Ptolemy's star is DM + 17°, 2318 (6.8 m.), measured 6.86 at Harvard. This may possibly be identical with the missing star of Ptolemy. If so, it must have diminished considerably in brightness, as Ptolemy rated his star 5th magnitude. The spectrum is of the 2nd type (H).

Aquarius.—With reference to Ptolemy's 6th star of this constellation, Al-Sufi says it "is the following of three stars in the left hand and precedes the 4th [β] . . . it is of the sixth magnitude; Ptolemy calls it the third, but in reality it is very faint [this indicates F 7, measured 5.89 at Harvard]. The 7th is the middle one of these three, and precedes the 6th inclining a little towards

the North ; it is of small fifth magnitude ; Ptolemy calls it fourth [this is evidently μ Aquarii]. Between it and the 6th there is about a span ($47'$). The 8th [ϵ Aquarii] is the preceding of the three and of large fourth magnitude ; Ptolemy says third. These three stars are followed by a star of the fifth magnitude [evidently ν Aquarii] . . . Ptolemy has not mentioned it. It is brighter than the 6th, which is in the hand, and between it and the 8th [ϵ], which is the most preceding and the brightest, there is about 3 cubits [7°] towards the East." Having plotted Ptolemy's positions, it seems to me that Ptolemy's 6th star was really ν , and that either μ or F 7 was not seen by him. As Ptolemy rated his 7th star 4 m. and his 6th and 8th 3 m., some considerable change of brightness must have taken place in these stars, as ν is now only $4\frac{1}{2}$ m., and F 7 only a bright sixth. The spectrum of ν is of the 2nd type (H, Pickering). Variation was suspected in F 7 by Gould. I found it very reddish with the binocular in Oct. 1882.

With reference to the stars numbered 23 to 29 by Ptolemy in Aquarius, there is some difficulty in identification. Al-Sufi says : " The 23rd is the first star at the outfall of the water [from the urn of Aquarius] below the four situated in the forearm and right hand, that is to say the 9th, 10th, 11th, and 12th [γ , π , ζ , and η Aquarii]. Between it and the 12th [η], which is the Southern of the four [in latitude], there are more than four cubits [$9^\circ 20'$]. It is of the fourth magnitude. There is between the 12th and this 23rd a star [κ Aquarii] of which Ptolemy has not spoken. The 24th follows the 23rd, a little towards the South ; between the two there is more than a cubit. The 25th follows the 24th by more than a cubit towards the North-east ; it is of small fourth magnitude ; Ptolemy calls it full fourth. The 26th is found below the 25th at about a cubit towards the South ; it is of small fourth magnitude ; Ptolemy says full fourth. Some of these stars must be in error with relation to their longitude and latitude indicated in Ptolemy's book, because in the sky they are seen otherwise than they are shown on the globe." Schjellerup identifies these four stars as follows :—23rd = F 67, 24th = λ , 25th = h , and 26th = ϕ . But from Al-Sufi's description I find that his 23rd star is λ , 24th = h , 25th = ϕ , and 26th = χ . The star F 67 is, now at least, below the 6th magnitude, and was probably not referred to by Al-Sufi. There is, however, considerable difficulty here in reconciling Al-Sufi's description with the positions given by Ptolemy. Let us carefully examine Al-Sufi's description of the other stars in this vicinity. He says : " The 27th, 28th, and 29th are three adjacent stars situated in a curved line below the 26th at a distance of nearly a cubit [$2^\circ 20'$], the convexity turned towards the East. As to the 27th it is the preceding and situated towards the North, the 28th is in the middle, and the 29th the most Southern. These stars are very close and all of the fourth magnitude." From this very clear description it is evident that the 27th is ψ^1 , the 28th ψ^2 , and the 29th ψ^3 Aquarii (Ptolemy's positions show only two stars in

this place). Al-Sufi's No. 26 must therefore be χ , and his 25th ϕ Aquarii. His 24th star seems to be h (83), and the 23rd λ , which Al-Sufi says is more than three cubits below (or south of) η Aquarii. As a cubit is $2^\circ 20'$, four cubits would be $9^\circ 20'$. The present distance between η and λ is about 9° , which is sufficiently near Al-Sufi's estimate, as he frequently rather over-estimates the distance between two stars. As to Ptolemy's stars, his positions when plotted seem to show that his 24th star is λ , the 25th= h , and the 26th= ϕ (as Schjellerup says). The 27th is χ , and the 28th and 29th appear to be ψ^1 and ψ^3 respectively. But Ptolemy's 23rd star, north of λ , seems to have disappeared, or has greatly diminished in brightness, as M. Flammarion points out ('Les Étoiles,' p. 430). At present there is a star (R.A. $22^h 50^m$, S. $5^\circ 31'$) not far from Ptolemy's position, which was measured 5.84 at Harvard (spectrum H). Schjellerup identified Ptolemy's 23rd star with F 67; but this is a mistake, as it does not at all agree with Ptolemy's position.

The explanation of the whole difficulty seems to be that Al-Sufi, not finding Ptolemy's 23rd star in the sky, gave the number to λ (Ptolemy's 24th), and continued the numbers on to the 29th, which thus came right, Al-Sufi having seen 3 stars in the ψ group, where Ptolemy saw only 2. Al-Sufi seems to have noticed that there was something wrong, as he states that the stars, as seen in the sky, did not agree with Ptolemy's positions. The star h has probably faded, as it is now about $5\frac{1}{2}$ m., although rated 4 by Ptolemy and 4-5 by Al-Sufi. χ has also probably diminished, as it is now slightly below the 5th magnitude. The spectrum of h is A, and that of χ is H.

Hydra.—With reference to the stars 13, 14, and 15 of this constellation Al-Sufi says: "The 13th is the preceding of three stars to the South of the bright one [α Hydræ] behind that towards the East; between it and the bright one there are 5 cubits nearly. The 14th is the middle of the three, behind the 13th; between them more than 2 cubits; it is of the 4th magnitude. The 15th follows immediately the 14th, inclining a little towards the North; it is of large fourth magnitude. Ptolemy says fourth. Between it and the 14th there is two-thirds of a cubit. Its latitude indicated in the book of Ptolemy is erroneous, because according to its longitude and latitude the distance between them should be a cubit and a half." From this clear description I find that Al-Sufi's No. 13 is ν^1 , 14= ν^2 , and 15= λ Hydræ. Having drawn a diagram from Ptolemy's positions, I find that his stars are 13= κ Hydræ, 14= ν^1 , and 15= λ . We must therefore conclude that Ptolemy and Al-Sufi saw only 3 stars where now there are 4. This was pointed out by M. Flammarion in his 'Les Étoiles' (p. 532), but my identifications do not agree exactly with his. κ was apparently not seen—or at least not referred to—by Al-Sufi, although it was rated 4 by Ptolemy, Tycho Brahé, Bayer, and Hevelius. At present it is about 5 m., and is probably variable. Spectrum A. If

Ptolemy did not see ν^2 it is probably variable also, as it was measured 4.72 at Harvard. It has been suspected of variable light. Its spectrum is of the 1st type (A).

With reference to the "extern" stars of Hydra, Al-Sufi says: "the 2nd is to the North of the 15th [λ Hydræ] . . . it is situated almost in the middle between the 15th and that in the Heart of the Lion [Regulus]. It is of fourth magnitude. Ptolemy says third." This description applies exactly to the star 15 Sextantis, but does not agree with Ptolemy's position. If Ptolemy's star was really 15 Sextantis it must have diminished in brightness, as it was measured only 4.50 at Harvard. If Ptolemy's star was not identical with 15 Sextantis it must have disappeared, as there is no star at present of even 5th magnitude in Ptolemy's position, the nearest being 22 Sextantis, which is a little south of Ptolemy's place and was measured 5.36 at Harvard. The spectrum of 15 Sextantis is A.

Lupus.—Ptolemy's positions of his numbers 16 and 17 in this constellation—as given by Al-Sufi—do not agree with Al-Sufi's description. Al-Sufi's stars are clearly χ and ξ Lupi, but Ptolemy's positions make 16 the preceding and southern of the two instead of northern as Al-Sufi says. Ptolemy calls the 16th, 4th magnitude, and 17th, 4-3. Possibly Ptolemy's stars may have been 16= ψ^1 and ψ^2 (seen as one star) and 17= χ Lupi. Al-Sufi does not mention ψ^1 and ψ^2 .

The 18th and 19th stars of Lupus lie a little south-west of 39 and 40 Libræ, and here, again, Al-Sufi's description does not agree with Ptolemy's positions. Al-Sufi says that the 18th is the Southern and preceding of the two, and the 19th the following. But Ptolemy's positions and descriptions, as given by Al-Sufi, make the 18th the southern and following, and the 19th the preceding. Al-Sufi rates the 18th, 6 or 6-7 magnitude, and the 19th, 5-6, but Ptolemy gives 4 and 4-3 respectively. The northern of Al-Sufi's stars seems to be f (2) Lupi, which was estimated 4.7 at Cordoba and measured 4.43 at Harvard; and the southern i (1) Lupi (4.95 Harvard). Perhaps these were the only two stars which Al-Sufi could see in this place, but, if so, one of Ptolemy's stars is probably variable. The spectrum of f is K, and that of i Lupi is F.

Corona Australis.—Al-Sufi says the 13th star of this constellation "is below the 12th [κ] towards the South, between them is a distance of a cubit and a half nearly; this star is situated between the less bright 12th and the bright 1st [θ], nearer, however, to the 12th. It is of the 5th magnitude." I cannot find any star in this vicinity which will suit Al-Sufi's description, the nearest being one (5.34 Harvard) about 1° south of κ . But Al-Sufi says "a cubit and a half," or $3\frac{1}{2}^\circ$. The present distance between κ and θ is $3^\circ 35'$, or about "a cubit and a half," so there seems to be no room for Al-Sufi's star. Ptolemy's longitude and latitude for this star—as given by Al-Sufi—are hopelessly incorrect.

It will be seen that most of the stars just considered have apparently diminished in brightness. They have all spectra of the 1st and 2nd types (A to H, Pickering), which, according to Sir Norman Lockyer's views of stellar evolution, are decreasing in temperature.

J. ELLARD GORE.

Octave Callandreau.

By the death of M. Callandreau the National Observatory, Paris, has suffered a grievous loss, and an active and valued worker has been removed from the field of dynamical and observational astronomy. The following slight sketch of his contributions to knowledge may interest readers of the *Observatory*.

On leaving the École Polytechnique in 1874, Callandreau joined the staff of the Paris Observatory, at first as an *aide-astronome*, and later (in 1881) as an *astronome-adjoint*. His work here for many years was the re-observation of the stars of Lalande's great Catalogue; but, side by side with this, he commenced almost immediately the publication of a long series of memoirs on various departments of Celestial Mechanics.

His first paper, suggested by a remark of Hermite's, was a note on the application of continued fractions to the calculation of Laplace's coefficients $b_i^{(n)}$ in the Planetary Theory. The expansions of the Planetary Theory always interested him, and when later, as a candidate for the Doctor's degree, he had to present a thesis to the Faculty of Sciences of Paris, he chose as subject the application of the methods of Gylden to the perturbations of the minor planets. Other papers relate to the calculation of inequalities of high order, and the expansion of the disturbing function when the inclination is large. Incidentally, several new results on Bessel and Legendre functions were obtained.

The Lunar Theory does not appear to have attracted him so strongly, though one of his papers is devoted to the establishment for Hill's equation of some properties which had previously been proved by Bruns for the equation of the elliptic cylinder.

To the theory of the figures of the heavenly bodies, Callandreau made many contributions: one of his papers, "*Sur la Théorie de la Figure des Planètes et de la Terre*," occupies 84 pages of vol. xix. of the '*Annales*' of the Observatory, and is of considerable importance.

In 1891 one of the prizes of the Academy was awarded to him for a memoir on Periodic Comets, which has been printed in vol. xx. of the '*Annales*.' In this he attempted to remove some of the difficulties which confront the capture theory.

Among his other works may be mentioned the completion, in one important detail, of Maxwell's famous memoir on the rings of

Saturn ; graphical methods for the prediction of occultations and the variation of elements ; and various improvements in mechanical quadratures.

In 1882 Callandreau formed one of the Hayti expedition to observe the Transit of Venus. In 1886 he became *Répétiteur* at the École Polytechnique, and in 1893 *Professeur du Cours d'Astronomie*; in the latter year he was elected a Member of the Institute of France, in the section of Astronomy, on the death of Admiral Mouchez. He was, with MM. Bigourdan and Radau, one of the editors of the *Bulletin Astronomique*, and to Callandreau must be assigned a share of honour for the high reputation of that journal.

M. Callandreau died suddenly on February 13, at the age of 51, leaving a widow and seven children.

E. T. W.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Sun-spots and Magnetic Storms.

GENTLEMEN,—

I notice on page 79 of your last issue that Mr. Maunder, in speaking of 19 great magnetic storms, is reported to have said that "7 coincided with the passage across the centre of the disk of one of the 19 great spots." Presumably the *central meridian* is intended, for no spot can transit the centre of the Sun's disk unless it is situated in an exceptionally low latitude, and most spots cross the meridian at considerable distances from the centre.

The reason I mention the point is that I wish to enquire whether distance from the centre of the disk at the time of meridian passage of a spot has any appreciable effect on the intensity and duration of the accompanying magnetic storm?

If the magnetic disturbances on the Earth are caused by streams of electrons, or charged corpuscles, projected from the very limited regions of the Sun's surface included in the umbrae of spots, it is evident that these streams must issue often at considerable angles with the direction of the Sun's radius. Yet, according to the results given in Mr. Maunder's interesting paper, this angle would seem seldom to exceed some 34° in the direction of solar longitude; for the storms usually begin from 34 hours before to 86 hours after the meridian passage of spots, which gives a total angle of 68° in longitude as the approximate limits of action* (or perhaps more than this if the duration of the storms is taken into account). If the average westerly position of the spots at

* It would be of interest to know whether the spots giving the greatest deviations from the mean position 26 hours west of the meridian are among those passing nearest to the centre of the disk. If not, then the above angle of 68° must be increased somewhat.

the commencement of the storms, *i. e.* about 26 hours west of the meridian, is wholly due to the time taken by the particles in passing from the Sun to the Earth, it follows that the streams may be equally inclined east or west of a radial line, giving thus 34° as the greatest inclination in either direction.

But how far are the effects felt in the direction of solar latitude north or south? Many of the "great" spots cross the meridian very far from the centre of the disk, especially when they appear in a hemisphere turned away from the Earth, as was the case last October. The two large spot-groups of that month never approached the centre of the disk nearer than about 25° ; the emanations which reached the Earth on October 12th and 31st must therefore have been inclined northward from a radial line by this same angle.

It would appear from the above that streams may issue from a spot probably in any direction within the limits of a cone, of which the apex of 68° or thereabouts is in the spot, and the axis is in the direction of a radius. But it seems to me it would be possible with the materials now available to get some notion of the average distribution of the streams within this conical region: are they, for instance, more frequent and intense in the direction of the radius?

It would be easy to classify spots under two heads—(a) those which approached within some arbitrary distance, say 10° , of the apparent centre, and (b) those which passed outside of this limit,—to ascertain whether there was a greater frequency and duration of magnetic storms accompanying spots of class a than those accompanying spots of class b.

Yours truly,

Kenley, Surrey,
1904, Feb. 8.

J. EVERSHED.

P.S.—The view that sun-spots are the loci of radiations capable of producing magnetic storms on the Earth was put forward by me in a general discussion on the cause of the darkness of spots, published in the *Astrophysical Journal*, vol. v. (1897). It seems, however, that for "radiation" we must substitute the *emanation* of material particles ejected from the spot at a speed of something like 1000 miles a second.

This newer theory is, however, quite in accordance with the view I expressed and still hold, *viz.* that sun-spots cannot be regarded as sinks in the photosphere filled up with cool, absorbing vapours. The darkness of a spot must rather be ascribed to the unveiling of an interior region where energy is dissipated by other means than by the emission of light.—J. E.

Eros and the Solar Parallax.

GENTLEMEN,—

In the *Observatory* for February, Mr. Hinks has an article on "Eros and the Solar Parallax." As I differ considerably from

some of the views he expresses there, I shall be glad if you will allow me to make a few observations on the subject.

Mr. Hinks begins by contrasting "the French and English schools of photographic methods." I do not admit the distinction which Mr. Hinks draws, but if the English school is to be limited to "obtaining the *relative* positions of objects on the plate" and "the avoidance as far as possible of the use of meridian places," I do not think it will get very far, with the one exception of work on stellar parallax. In the 'Astrographic Catalogue,' for example, the co-ordination of photographic with meridian results is of the greatest importance.

In the problem with which we are immediately concerned, the determination of the solar parallax from the photographs of Eros, it appears to me that, in Table IV., where he gives the differences between the tabular and observed places of Eros as derived from each photograph, M. Lœwy presents the results in the most simple and concise form. The results are at this stage intelligible to those who would not wish to wade through the earlier processes of the reductions. They are also in a form from which the mass of the Moon, as well as the solar parallax, may be obtained. The method of treatment would be like that adopted by Sir David Gill in his determination of the solar parallax from Victoria, Iris, and Sappho, a combined correction to the ephemeris and to the adopted system of star-places being formed.

It seems to me that the natural and convenient method of deriving from each photograph a correction to the ephemeris should not be departed from without a serious reason. The introduction of right ascensions and declinations gives a uniformity and simplicity without in the least impairing any relative accuracy. It would be worth a little extra labour in computing to adopt this form, had it been required. But the computations, for the most part, consist in the determination of plate-constants, refraction, factors for parallax, &c., and are identical whatever method of reduction is used.

Another advantage is that by following M. Lœwy's form of publication of the results, each observatory can reduce its own observations, almost to the end. By Mr. Hinks's method, the measures of photographs, taken near the same time by different observatories, must be dealt with together, just as Mr. Hinks is himself dealing with the observations of nine observatories for some days in November 1900. To my mind, it is desirable that each observatory should reduce its own measures as far as possible, at any rate to the stage of obtaining the apparent right ascension and declination of Eros as given by each photograph, and should not stop at the measured rectangular co-ordinates. No doubt some difficulties will arise from the assumption of different positions for the reference-stars, but I do not imagine that these will be at all formidable.

Yours faithfully,

Blackheath, 1904, Feb. 17.

F. W. DYSON.

Baron Plana.

GENTLEMEN,—

The recent appointment of a new Professor of Astronomy at Turin (Signor Boccardi) leads me to offer you a few lines on the most famous of his predecessors, Baron Plana, who died almost exactly forty years ago. Born at Voghera (between Pavia and Alessandria) in November 1781, Giovanni Antonio Amadeo Plana was educated at the Polytechnic School at Paris, and after holding an appointment at the Ecole Impériale d'Artillerie at Alessandria in Piedmont, was nominated Professor of Mathematics and Royal Astronomer at Turin, where he died on the 20th of January, 1864. His principal work, 'Théorie du Mouvement de la Lune,' appeared in three volumes in 1832, dedicated to Charles Albert *, King of Sardinia. Of it Airy remarks, in the preface to his 'Numerical Lunar Theory,' that, "viewed as leading to an algebraical result, this work was a great advance beyond all which had preceded it; and in numerical accuracy it is probable that something was gained." It was, indeed, superseded by the work of Delaunay; but in scientific progress it is well sometimes to remember earlier builders. Plana was nephew † of the famous Lagrange, who was a native of Turin, though the family was of French origin. His other mathematical investigations included the value of the great inequality between Jupiter and Saturn, the density of the Earth, motion in a resisting medium, the theory of refraction in the atmosphere, &c. The Gold Medal of the Royal Astronomical Society was awarded to Plana in 1840 for his work on the Lunar Theory.

Yours faithfully,

W. T. LYNN.

Blackheath, 1904, Jan. 28.

Aerolite Catalogues.

GENTLEMEN,—

In my letter with reference to Joshua's miracle the Catalogue of Dr. Bornitz is printed as that of Bonutz. Bornitz's is one of the most recent catalogues, and as regards the countries included in it I think it will be found redundant rather than defective. But I wish that the Germans, who are such experts in the "higher criticism," would expend a little of their critical acumen on these Catalogues of Meteorites, and cut down their lists to reasonable dimensions. I would decline to include Joshua's hailstones or the image of Diana at Ephesus in any such list; but Bornitz has actually duplicated Joshua, putting down one fall to A.D. 1460, and the other to a nearly equivalent date B.C. This is not the only instance in which I have found a fall duplicated in consequence of

* Father of the first King of Italy.

† In the obituary in the 'Monthly Notices,' he is said to have been his son-in-law; but as Lagrange had no children by either marriage, this must have been a mistake.

what was probably in the first instance a printer's error—possibly a slip of the pen on the part of a compiler who did not regard dates as of any importance. I have been (as some of your readers know) trying to ascertain whether aerolites, like ordinary meteors, fall in showers at particular seasons of the year, and have experienced great difficulty in arriving at accurate results owing to the uncertainty of many of the dates and the duplication of many of the falls. We are badly in want of a critic who will separate the wheat from the chaff. If Bornitz's Catalogue formed part of the Old (or New) Testament, some German critic would make mincemeat of it.

Truly yours,

W. H. S. MONCK.

Dublin, 1904, Feb. 5.

An Occultation of Venus.

GENTLEMEN,—

There was an occultation of Venus by the Moon, seen from this observatory, on the morning of the 15th December, 1903. The end of the phenomenon only was observed, a few minutes past 4 o'clock, but the exact time could not be noted. I have calculated the time as follows:—

Local Mean Time of beginning	14 ^d 15 ^h 6 ^m (astronomical)
„ „ end	14 16 11 „

The latitude of our observatory is 22° 34' 31''·2 N., and the approximate longitude 5^h 53^m 30^s E.

Venus was not far from its greatest brilliancy, and the sight of bright Venus emerging from behind the crescent Moon was unusual and very impressive.

Yours faithfully;

Presidency College Observatory,
Calcutta, 1904, Jan. 20.

PHANINDRA LAL GANGERLY.

The Great Comet of 1882.

GENTLEMEN,—

In an interesting letter under the above heading in your journal for August last, Mr. Lynn gives me the credit of being the first to get an instrumental observation of the Great Comet of 1882. As I do not wish to have any credit which is not due to me, I must ask you to allow me to state that, so far as I can ascertain, Mr. Finlay, of the Cape Observatory, was the first to fix the comet's position instrumentally. On consulting the proper authorities for the earliest recorded observations, I find that Mr. Finlay observed the comet at the Cape on Sept. 7^d 16^h G.M.T., whereas my first observation was on Sept. 8^d 7^h 51^m G.M.T. Mr. Finlay and Dr. Elkin made later observations on Sept. 8^d 16^h, and the next observer was Mr. White at Melbourne. Although I failed to be the first actual observer of the comet, I believe I may claim to be the first to detect it with the naked eye in full

daylight near the Sun. This discovery was made about Sept. 16^d 12^h Greenwich time, and the comet was immediately observed by means of the circles of the equatorial. An observation with the transit instrument failed in consequence of cloud. On Sept. 17^d 13^h 32^m G.M.T. it was again seen without a telescope, having shortly before passed its perihelion, and was then about three-quarters of a degree west of the Sun's western limb. I believe I was also the first to point out the circumstance that the comet was probably moving in an orbit resembling those of the great comets of 1843 and 1880.

Windsor, N. S. Wales,
1903, Dec. 28.

Yours faithfully,
JOHN TEBBUTT.

Annual Meeting of the Royal Astronomical Society.

GENTLEMEN,—

I should like, with your kind permission, to say a few words about a matter which the Author of the "Oxford Note-Book" refers to in the current number of the *Observatory*. I thoroughly agree with him that it might be worth while for the Fellows of the Royal Astronomical Society to consider whether the month of February is the best time for holding an Annual Meeting. Apart altogether from the exceptional character of the weather this year, we cannot, as a rule, expect any very great good fortune in the matter of favourable conditions in February; and I think that, in addition to the fact that bad weather may occasion us the disappointment of not seeing the distinguished stranger at all, we ought to consider the possibility of being able to give him as good a time as we can if he does turn up. It would not be safe to promise him any amusement at night in the way of astronomical observation, so we ought to arrange a good programme for him by day. London in May and June possesses many attractions to which even the most erudite astronomer cannot be wholly indifferent, and I would suggest that our Annual Meeting might be held in one of these months. There is no astronomical observation which is free from personal equation, and I hasten to confess mine. We country cousins do not like to have to come to town more than we can help before the season. The squire is busy just now with many matters, and the parson is fully occupied arranging his programme of Easter Services. This is a piece of genuine special pleading, perhaps; but I think there would be a larger and more thoroughly representative Meeting in May or June than in February.

I am, Gentlemen,

Melcombe-Bingham, Dorset.
1904, Feb. 16.

Yours faithfully,
ALEXANDER FOOTE.

The third Small Planet.

GENTLEMEN,—

Now that the small planets are numbered by the hundred, it is difficult for us to realize the enthusiasm inspired by the discoveries of the first four, at which the number remained for thirty-eight years after the discovery of Vesta in 1807. This year is the centenary of the discovery of No. 3, which was detected by Harding on the 1st of September, 1804, and afterwards named Juno. Karl Ludwig Harding (who had been born at Lauenburg in 1765) was at the time assisting Schröter at the observatory which he had constructed at Lilienthal, near Bremen, which was afterwards plundered and the greatest part of its contents burned during the war of 1813. Schröter died at Erfurt in 1816; Harding had removed before the calamity in question to Göttingen, where he became Professor of Astronomy, and died in 1834. He was a most assiduous observer, but Juno was his only discovery. The first to discover two small planets was Olbers, who found Pallas and Vesta in 1802 and 1807 respectively, the former because it was at the time near Ceres when that planet was recovered at its approach to opposition next after discovery. Olbers died at Bremen in 1840, so that none of the discoverers of the first four small planets lived to see the list increased beyond that number, though Herschel had suggested that the family might include thirty thousand members: more than half one thousand are now known.

Yours faithfully,

Blackheath, 1904, Feb. 13.

W. T. LYNN.

OBSERVATORIES.

WASHINGTON, U.S. NAVAL.—The Report from this Observatory for the year ending 1903, June 30, bears traces of the unfortunate disputes that have been associated with this institution in recent years. A new Superintendent, Capt. Colby Chester, U.S. Navy, was appointed, in place of Capt. Davis, on 1902, November 1. and he devotes some space in the report to explaining that the astronomical staff has been handicapped in the past by demands on their time for navy purposes, and shows that the energy and zeal of the members calls for more commendation than it has hitherto received. Capt. Chester makes some rather ambitious proposals for the establishment of branch observatories. He suggests that whilst the important chronometer-work should be done at Washington, observational observatories should be established at certain naval stations, and especially he proposes that at Tutuila, Samoa, there should be a small observatory for the purpose of observing a list of 500 of the 1597 stars adopted for publication in the 'Nautical Almanac' at the conference in 1896. This proposed

observatory to be in charge of a naval officer, having one of the assistants from Washington to make the observations. Another proposal relates to the eclipse of 1905. The Superintendent thinks it unwise to detach a large number of the staff to observe this, but proposes that the crews of navy ships should be drilled in making observations. He naturally quotes Sir Norman Lockyer on the subject, which only shows how careful a well-known man should be in setting a precedent. It is evidently Capt. Chester's view that official astronomy should be entirely in the hands of naval officers. It will be remembered that a Board of Visitors, composed mainly of civilian astronomers, was appointed in 1901 to investigate the affairs of Washington Observatory, and that the expenses of this Board were not voted in continuance in 1902. A paragraph which reads strangely in an official Report reverts to this fact, expresses approval of the discontinuance, and generally discredits the capacity of the Board. As for the astronomy done during the year, the Assistants appear to have been in a state of transition, and their individual reports are scarcely as full as usual. Prof. See has left the observatory, and Prof. Skinner, who formerly did meridian work, now has charge of the 26-inch equatorial, with which he observed comets, satellites, and small planets. Prof. Updegraff, who had charge of the new 6-inch transit-circle, has also left the observatory to go to the Naval Academy, and this instrument, as well as the 9-inch transit-circle, is in the charge of Prof. Eichelberger. Nearly 4000 observations were made with the new instrument, but there are no details as to its performance, as there have been in the last two reports. The new Altazimuth and the Prime Vertical Transit are still in the charge of Mr. G. A. Hill, with a member of the staff of the 'Nautical Almanac' Office who volunteered to help with the observations. It is stated that, in the ten years 1893 July 28 to 1903 June 30, 1018 observations of the passage of α Lyræ across the prime vertical have been made by one and the same observer, which is an average of slightly less than one observation in every four days. It is claimed that is a "world's record" for continuity of observation.

The 12-inch equatorial was used energetically throughout the year by Mr. King for observations of comets, asteroids, occultations, and other things.

MR. TEBBUTT'S OBSERVATORY, NEW SOUTH WALES. — The Report for the year 1902, which reaches us a little later than usual, is very similar to those of previous years, time-determination having been made on 103 nights and micrometer-measures of double stars, chiefly from Mr. Innes' Reference Catalogue, having been made on 82 nights, 1757 settings for position-angle and 1503 for distance in all. Mr. Tebbutt did not make any observations of Mr. Grigg's comet of July 1902, as unfortunately he

received information of the discovery too late, and his series of observations of Comet *b* 1902 (Perrine), of which he received news on October 16, was not begun until November 4, owing to an attack of influenza which prostrated him.

PUBLICATIONS.

THE ASTROGRAPHIC CATALOGUE*.—It is nearly seventeen years ago since the Conference was held in Paris which initiated the work of making a Chart and a Catalogue of the Heavens by photography, and some people who know that such a work was started, and who occasionally have evidence that it is in progress, ask when is to be the end. There are signs, in the recent publications from two observatories, that the end is at hand, or at any rate that some of the observatories engaged are working zealously, and that it is due to the magnitude of the whole undertaking that fruits of their labours have not been seen earlier.

The two observatories which have just begun publication are the Vatican at Rome, now under the direction of the Rev. Rodriguez de Prada, but of which the late Father Denza was Director when the astrographic work was begun; the other the observatory of Helsingfors, of which Prof. Anders Donner is chief; and these two books being before us for notice it seems convenient to begin with some elementary facts descriptive of the scheme of the whole work.

It is almost needless to say that the plates of the Astrographic Chart or Catalogue cover the sky in duplicate: one series, whose centres are on circles of declination of odd numeration, show all the stars in the sky, once and once only, if we consider each plate as bounded by the lines wherein it intersects its next neighbours; the other series, whose centres are on circles of declination of even numeration, similarly cover the sky, so that every star is shown at least twice.

Instead of regarding the stars as points on the surface of a sphere, as is the usual conception in astronomy, in the astrographic work we have them as points on a many-faced surface circumscribing a sphere, and this leads to a new system of co-ordinates. The position of a star will be defined if we know the position of a plate and the position of the star on that plate; i. e., in the new system four co-ordinates are necessary, the right ascension and declination of the plate-centre and the rectangular co-ordinate of the star on the plate, which are sufficient assuming

* *Catalogo Fotografico Stellare Zona Vaticana* (da $+54^{\circ}$ a $+65^{\circ}$ di declinazione) coordinate rettilinee e costanti di correzione. Volume I. (zone $+60^{\circ}$, $+61^{\circ}$, $+62^{\circ}$).

Catalogue Photographique du Ciel Zone de Helsingfors entre $+39^{\circ}$ et $+47^{\circ}$. Première Série. Coordonnées Rectilignes et Equatoriales. Tome IV. Clichés de 9^h à 12^h .

that the direction in space of the axes of co-ordinates are known. Endeavour is made that the axis of y shall be the projection of the meridian which passes through the plate-centre, and that the axis of x shall be at right angles to this. The defect from this is a plate-constant, called the orientation, to be determined from the measures. Other plate-constants are the error of position of the actual centre and the scale of the plate.

A star position given by these co-ordinates can be transformed by comparatively simple geometry into the older co-ordinates, right ascension and declination; but the Committee which controls the astrographic work has not said that this is necessary. At present the resolutions passed at the Conferences have ordained simply that the original measures of the rectilinear co-ordinates shall be published, with sufficient data of the nature of plate-constants for their reduction, but they do not prohibit any participating observatory from publishing more than this, a state of things which does not tend to uniformity. The conferences, too, have not settled any uniform size of volume nor scheme of publication as is the case with the Catalogues of the *Astronomische Gesellschaft*, so that the complete Astrographic Catalogue will be somewhat heterogeneous.

These two books are illustrative of this want of uniformity. The Roman zone extends from declination $+54^\circ$ to $+65^\circ$, and this first volume contains some of the plates, not all, and not even consecutive plates, in the zones $+60^\circ$, $+61^\circ$, and $+62^\circ$. The portion allotted to Helsingfors comprises the zones of plates whose centres are at dec. $+40^\circ$ to $+46^\circ$, and the volume now published contains all the plates in those zones from R.A. 9^h to 12^h . This volume, it will be noticed, is numbered IV., the Helsingfors plan being to publish a first series of eight volumes such as this, in which the plates are in order of R.A., and then a second series of 6 volumes, in which the stars are all collected strictly in order of that co-ordinate.

There are many points to be commended in the Helsingfors work. The plates are numbered in order of position, *e. g.*, the centre of No. 1 has $\alpha = 0^h 0^m$, $\delta = 40^\circ$; No. 2, $\alpha = 0^h 0^m$, $\delta = 42^\circ$ up to 46° ; No. 5 has $\alpha = 0^h 5^m$, $\delta = 41^\circ$, and so on. At the head of each plate, above the columns which give the measures, are given a great many facts about the taking and measuring of the plate, and all the constants necessary for correcting the co-ordinates. Moreover, in the columns there are given, not only the measured co-ordinates, but also these constants corrected by application of the plate-constants, and beyond that these are converted into equatorial co-ordinates, Right Ascension and Declination for epoch 1900.0. This is going much beyond the scheme laid down by resolution, but not too far; the only objection that can be made is the want of homogeneity that it entails, for many observatories will not be able to go so far, perhaps from want of funds, or perhaps because they may prefer to finish the work in less time, even at the sacrifice

of completeness. But the volumes already published seem to have one defect. It is difficult to pick out the different measures of the same star from the various plates on which it occurs. As has been said, it must be on two, and it may be on three or more, but there is no ready means of knowing on how many. The conversion into equatorial co-ordinates, as is done in the Helsingfors work, overcomes the difficulty largely, but the plan adopted at Greenwich, of measuring plates only up to the lines of intersection with their neighbours, so that one star is measured twice and twice only, and then the printing these duplicate measures in one line of the catalogue, has its advantages. At the moment there is no means of testing the value as a star catalogue of the book from the Vatican Observatory, but its introduction states that the probable error of an equatorial co-ordinate deduced from it is $\pm 0''35$. Inspection of the duplicate determinations of R.A. and declination of any star shows that the places of the Helsingfors catalogue are exceedingly accurate, and the whole volume gives evidence of great care and skill. It contains 125 plates, and each of these contains on the average about 100 stars; so that allowing for two, three, or more measures of any star, we have here the accurate positions determined of perhaps 5000 stars in this region of the sky, which is about one-hundred and sixtieth of the whole sphere.

H. P. H.

NOTES.

COMET NOTES.—*Ast. Journ.* 555 contains a definitive orbit of Comet 1845 III. by H. A. Peck. This comet was discovered by Colla and Bond, and was plainly visible to the naked eye. It was at one time thought to be identical with the comet of 1596 observed by Tycho, but it is now shown that this is untenable, and that the orbit is sensibly a parabola, as follows:—

T	1845 June 5	6897 Paris M.T.	
ω	75° 48' 17"		} 1845.0.
Ω	337 48 48		
i	131 4 44		
$\log q$	9.60323		

The following elements of Denning's Second Periodic Comet (1894 I.) are by Dr. P. Gast:—

Epoch	1894 March 28.0	Berlin M.T.
M	6° 10' 45"	
π	130 37 9	
Ω	84 22 20	
i	5 31 46	
ϕ	44 17 55	
μ	478".30	
$\log a$	0.58021	
Period	7.419 years.	

The comet was not seen in 1901, being unfavourably placed, but it should be readily visible at the next return about the end of 1908.

A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered :—

Planet.	Date.	Discoverer.	Magnitude.	Place.
NC	1904 Jan. 11	Wolf.	12.3	Heidelberg.
ND	27	Dugan.	11.8	"
NE	1903 Nov. 26	Hirayama.	12.0	Tokio.
NF	Dec. 15	Peters.	..	Washington.
NG	Details not yet to hand.			
NH	1904 Feb. 15	Charlois.	10.5	Nice.

NH is worthy of note as being considerably brighter than most recent discoveries.

NF is probably identical with DW, discovered in 1898.

The following planets have been named :—360 Carlova, 456 Abnoba, 462 Eriphyla, 482 Petrina, 483 Seppina, 484 Pittsburghia, 488 Kreusa, 493 Griseldis, 503 Evelyn, 507 Laodica.

The variability of two planets is announced : 7 Iris is said to range through a quarter of a magnitude, the period being 6 hours, and 135 Hertha through half a magnitude (period not stated). It will be remembered that the variable light of the asteroids was announced a very long time ago, then discredited, and revived in the last three years, the case of Eros attracting special attention.

A. C. D. C.

THE LONGITUDES OF THE INDIAN SURVEY.—A hundred years ago, in the early days of the Indian Survey, the longitude of the Madras Observatory was believed to be $80^{\circ} 18' 30''$ E. of Greenwich, and the lines of longitude on the sheets of the Atlas published by the Survey were laid down in accordance with this datum, but the Standard Sheets, which, presumably, were begun later, were marked in accordance with a later value of the longitude, $80^{\circ} 17' 21''$, and this system remained in use until quite recently, although it was known, from later determinations, that this last value was too large by about $2\frac{1}{2}'$, footnotes being added to the maps in correction. This led to a re-determination of the longitude of Madras in 1895*, with result $80^{\circ} 14' 47''.06$. It has since† been decided that this value shall be adopted, or rather, since the difference of longitude between Madras and Kalianpur (which is the origin of the Survey), determined astronomically, is $7''.14$ less than the difference measured by triangulation‡, it is

* See *Observatory*, vol. xviii. p. 330, vol. xxi. p. 90.

† In a paper by Major Burrard, dated 1903, lately come to hand, it is stated that Col. Gore's order on this point is dated 1900 May 17.

‡ $2^{\circ} 35' 29''.49$ astronomically, $2^{\circ} 35' 36''.63$ geodetic.

decided that the longitude of Kalianpur E. of Greenwich shall be adopted wholly from astronomical determinations, *i.e.* $77^{\circ} 39' 17'' \cdot 57$, but that Madras shall be measured from this by triangulation, so that notes to the following effect are now on the maps and sheets of the Survey of India :—

The longitudes are referable to the Greenwich meridian, taking that of Madras Observatory as $80^{\circ} 14' 54''$ East, the most recent geodetic value.

AN IMPROVED SUN-DIAL.—It may seem strange that it should be necessary to use a sun-dial for scientific purposes, but among the places where tide-gauges are in use in India there are some so far removed from civilization that, as there is no telegraph-wire and no observatory near, and the assistants in charge of the gauges are scarcely efficient enough to use a transit instrument, there is some difficulty in knowing the necessary local time. Col. Strahan, the Deputy Surveyor-General, has therefore proposed the use of a sun-dial, but since an objection to this instrument arises from the fact that the edges of the shadow cast by the style are always rather badly defined, he has devised a simple form of dial, its chief feature being a board whose plane is set parallel to the Earth's axis, and in this board is cut a very fine slit, in a north-and-south direction, so that the shadow of the style in the usual form is replaced by a thin line of light. Explicit directions for making and adjusting this are given in 'Professional Papers of the Survey of India,' 1903, No. 7.

THE ROYAL SOCIETY.—Major S. G. Burrard, Superintendent of the Indian Trigonometrical Survey, Dr. Charles Jasper Joly, Royal Astronomer for Ireland, and Mr. Gilbert T. Walker, Meteorological Reporter to the Government of India, are in the list of persons selected by the Council for election as Fellows of the Royal Society.

THE COUNCIL OF THE ASTRONOMICAL SOCIETY.—It is worth recording, for the case is unusual, that the names of the Officers and Council given on page 113 as elected for the ensuing year are not exactly in accordance with the list suggested and printed by the outgoing Council. The name of one Member of Council differs in the two lists. Whilst the ballot was proceeding, attention was publicly called to the fact that a list of names differing from that issued by the Council, but similar in appearance, was being distributed in the meeting-room. The Fellow who called attention to the matter evidently thought this was a piece of sharp practice on the part of somebody.

A NEW OBSERVATORY.—An observatory has been established at Zagreb, the capital of Croatia (Hungary), under the direction of Prof. Otto Kucera. This institution, which is an offshoot of the Croatian Philosophical Society established in 1887, aims at doing

good astronomical work as well as popularizing the science in Croatia. It already possesses equatorials of 6.4 inches and 4.25 inches aperture, as well as other instruments, and with these it is proposed to observe the Sun and planets, variable and coloured stars.

THE OCCULTATION OF ALDEBARAN.—Unfortunately on the evening of February 24 the sky was completely overcast at Greenwich, so that neither the Moon nor the star was seen; but there will be other occultations of this star this year and in 1905 and 1906, the next occasion being on July 9. The special interest of this phenomenon lies in the fact that Aldebaran has experienced "anomalous" occultation more than any other star. The records of these go back a long way. In 1699 the star was occulted, and an observer at Marseilles noted that it did not disappear until some seconds after it had touched the limb. From this time forward the phenomenon seems to have been especially looked for by French astronomers—a collection, made by Sir James South, of such observations by De la Hire, Le Monnier, Messier, and others, will be found in the third volume of the *Memoirs R.A.S.*

"THE greatest living astronomer." Most persons would feel some diffidence in putting a name to this individuality. Not so the *Daily Mail*, who unhesitatingly applies the phrase to Sir Robert Ball, the author of an article on Prof. Hale's work in the issue of that paper for February 5.

On April 1 next, Prof. Wilhelm Foerster, who will be then 72 years of age, and has served the Royal Observatory at Berlin for 49 years (10 years as Assistant, 39 years as Director), will retire from his office. He will be succeeded by Prof. Hermann Struve, now Director of Königsberg Observatory.

WE have received a copy of the third edition of Mr. W. T. Lynn's 'Astronomy for the Young,' recently issued by Messrs. Sampson Low & Marston. This is one of the series of handy little astronomical books sold for sixpence that we have before had occasion to eulogize.

THE Madrid Correspondent of the *Times* says that the Spanish Government are already being urged to prepare for the solar eclipse of 1905 by sending a mission of astronomers to foreign observatories to study the latest methods of investigating the phenomenon.

SIR DAVID GILL is expected in England during the month. He is coming home for reasons connected with his health.

SIR WILLIAM HUGGINS was 80 years old on February 7.

THE rainfall at Greenwich in February, up to 9 o'clock in the morning of the 28th, amounted to 2.53 inches, which is 1.05 inches above the average. There were nine days on which it did not rain.

THE next Meeting of the Royal Astronomical Society will be on Friday, March 11; of the British Astronomical Association on Wednesday, March 30.

FROM AN OXFORD NOTE-BOOK.

OXFORD has been thrilled by a great fight—not the Russo-Japanese war, but an academic fight about Greek. Shall some knowledge of Greek be demanded of *everyone* who comes to Oxford? That is the real question at issue. In the minds of different people it takes different shapes. There are many who regard it as identical for all practical purposes with this:—Shall the study of Greek disappear from off the face of the earth? And they naturally vote *non placet!* as I hope we all should if that were really the question. The actual proposal before Congregation was, however, simply that in the case of some students who are going to take honours in science or in mathematics, and who, though men of ability in other directions, find the study of Greek uninteresting and, in plain words, a mere nuisance, some other evidence of preliminary education should be required. That there are such men is quite certain, just as there are men who would never gain anything from any amount of work at music or at cricket—the time spent would in their case be worse than wasted. But Oxford is slow to believe that Greek has not the same virtues for everyone; and also, as above remarked, terribly afraid that this is the beginning of the end of Greek; and so the preliminary proposal for reform in this direction was only carried by two votes—164 to 162. What will ultimately happen is therefore by no means certain, for there are several stages to come. It is noteworthy, however, that for the first time in history a resolution in this direction has been carried at either Oxford or Cambridge; previous proposals have always been thrown out by large majorities of 2 or 3 to 1, excepting only a proposal made about a year ago here in Oxford, which was lost by a majority so small as to encourage another attempt on slightly different lines.

To those who are not familiar with the English educational system, it may perhaps be explained that what is called “Compulsory Greek,” i. e. an examination in Greek which every student must pass on coming to the Universities of Oxford or Cambridge, has a considerable effect on our public school system, viz. so much time and attention is devoted to the classics that it is impossible

to devote proper attention to mathematics, natural science, and modern languages—so, at any rate, the headmasters of our great public schools say. It is believed that they will welcome some relaxation of this necessity, which would enable more of a scientific education to be introduced into schools. The Royal Society, acting on this belief, has recently addressed a memorial to the Universities urging them to consider the undue neglect of science in our public schools at the present time, and whether they can take any steps to remedy it. The memorial was sent to all English Universities, and the copy of it addressed to Oxford (which was identical with the others) appeared in the *Times* of February 19. The memorial is the outcome of the Address of the President of the Royal Society in November 1902, of which a paragraph was quoted in these Notes in January 1903.

IN the introduction to his edition of Marco Polo's travels, Col. Yule quotes from Pietro of Abano a curious reference to the greater Magellanic Cloud:—"In the country of the Zingi there is seen a star as big as a sack. I know a man who has seen it, and he told me it had a faint light like a piece of cloud, and is always in the south. I have been told of this and other matters by Marco the Venetian, the most extensive traveller and the most diligent enquirer I have ever known. He saw this same star under the Antarctic; he described it as having a great tail, and drew a figure of it *thus* [the figure is something like the capital Ω]. He also told me that he saw the Antarctic pole at an altitude above the horizon apparently equal to the length of a soldier's lance. . . ." Upon the accounts of the traveller himself the editor has the curious comment that "of scientific notions, such as we find in the unveracious Maundevile, we have no trace in truthful Marco," who estimated his latitude by the height *in cubits* of the Pole Star. "Such expressions convey no notion at all to such as have had their ideas sophisticated by angular perceptions of altitude."

THE above is from a Cambridge correspondent, to whom I make due acknowledgments. He also sends me the following note on a point to which attention has already been drawn in these notes. His comments are more vigorous than mine were, as the irritating cause of mistakes affects him more directly; but allowing for this difference of detail, I sympathize entirely with what he says. Cannot steps be taken to prevent the evil growing? The *Astronomische Gesellschaft* could do much to correct one source of error which may apparently cause serious inconvenience and even actual loss at times. But I will leave the matter to my correspondent:—

IN the way of aggravating pedantry there is nothing to beat the insistence of continental astronomers in referring to Harvard

College Observatory as Cambridge. The authorities at Harvard do all they can to discountenance the practice, without any effect upon people with a continental passion for consistency. The *Astronomische Gesellschaft* insisted on confusing their zone catalogues with the two titles "Cambridge (U.S.)" and "Cambridge (Engl.)," with the result that an editor who ought to have known better actually altered in a reference the name "Graham" to "Rogers"! And it gives us a salutary idea of the relative glory of two institutions to find that "U.S." flows naturally from the pen of the correspondent who writes to an astronomer at Cambridge. To the more obscure place the post has at last brought a postcard which had travelled from Tashkend *via* Boston (U.S.A.), and also a letter addressed by the secretary of a great observatory, who had sent things right on a dozen occasions, but on the thirteenth, when time was really of vital importance, sent a batch of results across the Atlantic, whence they returned, just too late. It is also sad to note that certain otherwise admirable Circulars are not content with misnaming Harvard, but have improved upon the names of other places; at the identity of the Mount Hamilton Observatory we can guess, but who can identify, without pause, the Observatory of Williams Bay?

THE answers to examination questions reproduced from the *Daily Telegraph* on p. 73 of this year's Notes have apparently been appreciated by several correspondents, who have sent me similar ones; and they have even been translated into German! Here are the new contributions. They are not strictly astronomical, but they were all sent by astronomers of repute. I trust it is not necessary to disclaim responsibility for the religious views expressed in some of them:—

We are taught not to be anxious for the future, as the future will come in time.

We are taught in the Sermon on the Mount not to think of the future, because the evil we do in one day is sufficient.

Sidereal time is the time it takes the Moon to come round again to a star that it was at about twelve hours ago.

The nearest fixed star is SYREUSE, but I forget the spelling.

An epithet was formerly an inscription on a tombstone: afterwards applied to any short, pithy, wise saying: illustration, "A little knowledge is a dangerous thing."

(Define Vengeance.) Vengeance is a mean desire to pay back: illustration, "Vengeance is mine, saith the Lord."

(What is the cause of the Rainbow?) This subject, as I understand it, is not very well understood.

Cæsar was the female god of the Phœnicians.

"My thoughts cling to the mouldering paste."—*Longfellow*.

They ascended Vesuvius to see the creator smoking.

THE Baltimore lectures, delivered by Sir William Thomson at the Johns Hopkins University after the meeting of the British Association in 1884, have been published by Lord Kelvin in 1904. They were taken down in shorthand by one of his devoted audience, Professor Hathaway, and reproduced in a beautifully-written MS. by a papyrograph process. From these notes the distinguished lecturer has gradually evolved the book just printed by the Cambridge University Press—just about three times the length of the original. But possessors of the papyrograph will rejoice to find that they have something for their money which is not in the new volume, viz. the farewell verses written by one of the audience to the lecturer. Nor will it lessen the value of the original if I reproduce them here for the benefit of such as may not have seen them. They contain some long words which are chiefly due to Rankine, as Sir William Thomson (what memories the old name calls up!) was careful to explain. In one lecture he gave a most interesting little *excursus* on the derivation of words from the Greek, and pointed out that Rankine need scarcely have invented "cyboid" when "cubic" would have done equally well. Here are the verses and the apologetic footnote with which Professor Hathaway printed them.

The Lament of the 21 Coefficients in parting from each other and from their esteemed Molecule.

An aeolotropic molecule was looking at the view,
Surrounded by his coefficients, twenty-one or two,
And wondering whether he could make a sky of azure blue
With platitatic $a\ b\ c$ and thlipsinomic Q .

They looked like sand upon the shore with waves upon the sea,
But the waves were all too wilful and determined to be free;
And in spite of n 's rigidity they never could agree
In becoming quite subservient to the thlipsinomic P .

Then web-like coefficients and a loaded molecule,
With a noble wiggler at their head, worked hard as Houghton's mule;
But the waves all laughed and said, a wiggler thinking he could rule
A wave was nothing better than a sidelong normal fool.

So the coefficients sighed and gave a last tangential skew,
And a shook hands with b and c and S and T and U ;
And with a tear they parted, but they said they would be true
To their much-beloved wiggler and to thlipsinomic Q .

Signed (g, f), a cross coefficient now annulled.

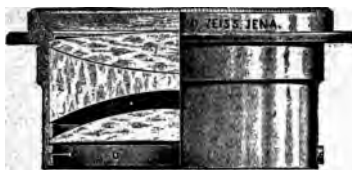
[President Gilman passed favorable verdict upon the versification, Sir Wm. Thomson said the mathematics seemed all right, and the coefficients unanimously concurred in the sentiments expressed. I therefore consider its insertion justifiable, even in a more solemn and heavy scientific work than this purports to be.—H.]

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A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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VOL. XXVII.

APRIL, 1904.

No. 343.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 March 11.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker. Eighty-five presents have been received since the last Meeting. Among them is a volume of the 'Annals of the Cape Observatory,' the Revision of the Photographic 'Durchmusterung,' Parts 1-3, and also Schröter's 'Beiträge zur den neuesten astronomischen Beobachtungen,' presented by Dr. Dreyer.

The President. We have also received some more contributions to our gallery of Presidents. One is a present from Sir Robert Ball of Prof. Brinkley; another from Dr. Rambaut of Manuel Johnson, who was the Radcliffe Observer in the middle of the last century. Then we have portraits of Prof. Pritchard and Mr. Stone, and of Admiral Smyth, which has been given us by Mrs. Baden-Powell. When the enlargements are made, the collection will be practically complete up to modern times, and there will be no gap in it, which is a very satisfactory result. I should like to mention what, perhaps, Fellows may not know, that these pictures represent a great deal of work and devotion on the part of Mr. Wesley. He is responsible for the artistic taste shown in most of them, and I am sure you will return your thanks to the donors of these valuable presents.

The President. We have to-night four papers dealing more or less directly with the Lunar Theory, and I propose to take these first. There is one by Prof. E. W. Brown, which Mr. Whittaker will read; there is another from Mr. Nevill, which Mr. Dyson will read; and then there is a note on some instrumental errors by

myself, which I shall read in order that Mr. Cowell may have an opportunity of replying to it when he reads his paper.

Mr. Whittaker read the paper by *Prof. E. W. Brown*, which dealt with a comparison of the observed and calculated values of certain quantities occurring in the Lunar Theory. The paper was a sequel to one which had appeared in the Society's 'Monthly Notices' in the preceding year; at that time the final verification of terms due to the Sun's action was not complete, but this work was now accomplished, and an exhaustive examination had been made of the various constants used, and of the effects not due to direct solar action. The results of the comparison were given in the paper.

Mr. Dyson then read *Mr. Nevill's* paper on the comparison between the purely theoretical and the observed places of the Moon. This, to some extent, is a criticism of Mr. Cowell's note on the same subject. Mr. Nevill, in considering Mr. Cowell's paper, remarked that he himself had applied in some cases slightly different corrections, but in the main he agreed with those applied by Mr. Cowell. Mr. Nevill also said that a comparison of observations with Hansen's tables showed differences which he was quite unable to explain by gravitational theory. The paper contained tables of the errors of Hansen's tables from 1750 to the present time, both Hansen's tables uncorrected and with the corrections which Mr. Cowell gave in his note. The paper was so much a mass of figures that it was impossible to say much about it at the Meeting.

The President. The note I shall read had its origin in a little suggestion I made some time ago for photographing the Moon amongst the stars, with a view to determining, if possible, the errors of the tabular place more free from instrumental errors. It is well known that observations of the Moon made on the meridian are made at full Moon in a dark sky, but that near the quarters the observations run into daylight, and there is accordingly quite probably a systematic error due to the different conditions which is at present of unknown amount. It occurred to me that there is a rough check upon the total amount of such an error. The observations made with the altazimuth were directed to be made at Greenwich in a dark sky as much as possible. One of Airy's instructions was that the observer must wait until the sky was sufficiently dark, and hence these observations would be affected with a different kind of error from meridian observations. The observations from 1847 to 1861, both with altazimuth and transit-circle, are compared with Hansen's tables in the Appendix to 'Monthly Notices,' vol. 1. Mr. Cowell has been showing that Hansen's tables are affected with several errors, but for our purpose these do not matter now, because the error will be the same for the two instruments. One of the points made in the paper is that we may have some little confidence in such a comparison because the altazimuth seems to have behaved consistently throughout the

whole period. (Blackboard.) Hence we may for present purposes compare the old transit instrument and the new transit-circle with each other by comparing each with the altazimuth, and the difference between the two meridian instruments is quite striking. I should have said that a simple difference of diameter does not come in; we should be prepared for that, and that has been allowed for as a constant difference before and after full Moon. But there is a progressive change in the difference between the two meridian instruments, starting from zero at full Moon and reaching $1''.2$ at the quarters. A systematic error of this kind would have a very sensible effect upon the parallactic inequality and other inequalities which Mr. Cowell has been discussing. I draw attention to these systematic errors in the hope that I may elicit from Mr. Cowell some statement as to how he proposes to deal with them.

Mr. Cowell said that he felt grateful to Mr. Nevill for the criticisms that were contained in his paper. These criticisms would undoubtedly be most useful to him. His own November paper, it must be admitted, contained some blunders, and he was glad to have them pointed out. Nothing in his analysis hitherto was, however, in the least affected. He thought also that Mr. Nevill had mistaken a statement of work considered advisable for the future for a statement of work already done.

Mr. Cowell then explained on the blackboard the two underlying principles of analysis of the errors of the Moon. In the first place, there were errors of nearly equal period, and hence it was necessary to extend the analysis over a long period of time to separate such errors; in the second place, the distribution of the observations round the times of full Moon, and the absence of observations at new Moon, made it necessary to consider simultaneously with any argument ϕ the allied arguments $\phi \pm D$, D being the elongation of the Moon from the Sun. Since the average value of $\cos D$ was $-\frac{1}{2}$, it followed that a real correction $\beta \sin \phi$ not only appeared as an inequality $\beta \sin \phi$ in the errors, but also as $-\frac{1}{2}\beta \sin(\phi + D)$ and $-\frac{1}{2}\beta \sin(\phi - D)$, and conversely an apparent inequality of argument ϕ might be due to a real inequality of argument ϕ , $\phi + D$, or $\phi - D$.

Mr. Cowell next explained his results. These were:—

- (i) A correction to the eccentricity of the Moon's orbit.
- (ii) A correction to the evection, disproving Hansen's assumption of ellipticity in the Moon's figure.
- (iii) A correction to the term, argument $g - D$, allied to both the anomaly and the evection. This correction represents an error in Hansen's tables, and had already been corrected in the 'Darlegung.'
- (iv) A correction to the annual equation, apparently rather greater than required by theory. Possibly, however, when Prof. Brown's calculations were complete, theory might verify this correction.

- (v) The allied term with argument $g - 2D + w - w'$ corroborated the preceding correction.
- (vi) The other allied term, $g + w - w'$, could not be distinguished from a Venus term, $g + 2\varpi - 3V + 5E$, with less than a hundred years' observations. It was necessary to put this aside for the present, until the Airy period, 1750-1851, was corrected.
- (vii) The coefficient of the Jupiter evection term was $1''.1$. Prof. Newcomb had assigned $1''.5$ to this coefficient in 1876, and Radau and Hill had assigned $0''.9$ from theory.
- (viii) A second Jupiter term, $0''.3 \sin(g + 2\varpi + 3J - 7^\circ)$, had been verified.

Coming now to Prof. Turner's criticism of his paper last December, on the parallactic inequality, Mr. Cowell said he did not think that 14 years' observations with the altazimuth was sufficient evidence to set against 50 years with the transit-circle. A quantity of $0''.3$ could hardly be obtained with any certainty from so short a period as 14 years. As evidence of this he pointed to the discrepant values of the eccentricity that had been obtained at various times.

Mr. Dyson said he had nothing to say exactly on the paper, but he would like to emphasize the point that Mr. Cowell was engaged on a big piece of work. It was a matter for congratulation that while Prof. Brown was engaged on theory, Mr. Cowell was engaged on the comparison of theory with the observations. He did not think they could have anything more satisfactory for lunar theory than that work on such lines should be going on simultaneously.

The President. With regard to Mr. Cowell's remarks on my paper, it must be remembered that Mr. Cowell has only had an opportunity to glance at it, and therefore spoke with imperfect knowledge. I think it will be rather difficult to explain any of the effect by bringing in the tables. Further, the effect is a very much greater one than $0''.3$; it amounts to $1''.2$. It is a purely instrumental one, and has nothing to do with variation or parallactic inequality, but purely with the observer's habits, so far as my observations have allowed me to come to any conclusion. I think it will be necessary to make some investigation of these serious peculiarities before we can say whether we have arrived at any knowledge of these particular terms having a period of one month. There is, however, a great deal of Mr. Cowell's work which is not touched by this criticism at all, and I am sure we shall all join with Mr. Dyson in congratulating him upon the great progress he had made, especially as Prof. Brown is also working at another department of this very important preparation of a new lunar theory.

The President. The Astronomer Royal is able, I understand, to say something to-night about the first volume of the Greenwich Section of the Astrographic Catalogue, which is now practically completed.

The Astronomer Royal. We at Greenwich are—I am sure Mr. Hollis particularly is—glad that this volume of the Greenwich Astrographic Catalogue, including various discussions in the Introduction, has now been finally sent to the press, and will be, I hope, in the hands of the Fellows shortly. Meanwhile, this seems a good opportunity to call attention to some points which have been discussed in it, and also to give a general account of the contents of the volume. It is proposed to publish the measures of the photographs taken at Greenwich for the Greenwich section of the Astrographic Catalogue—that is, from declination $+64^{\circ}$ to the Pole—in two volumes, besides which it is proposed to have a supplementary volume, in which there will be a catalogue of Right Ascensions and Declinations deduced from the measures with definitive plate-constants. In this volume is contained the first half—that is, the measures from 64° to 72° inclusive. This area covers 1077 square degrees and contains 80,000 stars. By that I mean there are that number of separate stars in that region of the sky. The method adopted at Greenwich of measuring overlapping portions of two plates at the same time by the duplex micrometer ensures that there is no duplicating of stars in measuring the two series of plates, and it is quite easy to count up the number of actual stars which have been measured. For the remaining half, as we have now made the measures from declination 72° to 83° , it is hoped that the whole work of measurement will be completed by the end of this year. We can make a pretty fair forecast as to the exact number of stars that there will be in the Greenwich section, which may be put at about 179,000. Up to the present time in the second volume 85,000 odd stars have been measured in 886 square degrees, and about 150 square degrees remain to be measured, so that we shall have considerably more stars in the second half than in the first, which, as I have said, contains about 80,000. This increase is not altogether due to the increased sensitiveness of the plates. It may be partially due to that, but there is evidence that from the lowest zone, going northward, each zone shows an increasing number of stars per square degree. From tabulation of the number per square degree for each zone, we have found that it has a maximum at 76° Dec. This effect is probably due to the Milky Way. At Dec. 82° and 83° the number has fallen off, and it will be interesting to see how it goes as the measuring approaches the Pole. There are several matters dealt with in the Introduction, which are referred to in the paper I now contribute, and may be considered under the heads of the determination of the position of stars on the plates and the determination of their photographic magnitudes. As regards the determination of position, it was thought necessary to determine the personality of the measurers. That work was taken up soon after the measurement began, but the discussion could not be conveniently published then, and was deferred until it could be included in this volume. I should explain that the measurement of the plates was done at

the beginning by making measures by different measurers in one position of the plate in the micrometer. That was in 1895-96, but as a result of the Astrographic Conference in 1896, it was decided to duplicate the measures so as to get increased accuracy, and advantage was taken of that proposal to measure the plates in the reversed position in the micrometer—that is, the plate is turned through 180° , and this, as I will show presently, practically eliminates any personality. The fact that reversing the plate through 180° virtually eliminates the personality of the measurer has a bearing on the discussion which has taken place recently between M. Lœwy, Mr. Plummer, and Mr. Hinks on the question of how to obtain the greatest accuracy in measurement. This table of figures now on the screen illustrates the method of determining a measurer's personality in either position of the plate. Equations of condition were formed in which the known quantities were differences between actual measures of stars in the two positions of the plates. These equations were solved, and this next table shows the results deduced from the discussion of many thousands of measures. It will be seen that in almost every case a measurer's personality is about the same magnitude when measuring "Reversed" as when measuring "Direct," but of opposite sign. Another subject in the Introduction is the determination of the photographic magnitude of stars. The point is to find out what the photographic diameter is actually equivalent to in photometric magnitudes. It was thought better to give the actual measured diameters; or, in the case of very faint stars, not actually the measure of the diameter, but rather the diameter of an equivalent black image, allowance being made by the measurer for want of density in the image and shading off at the edge. I think very consistent results have been got, and it has been found that the scale can be carried on to quite the faintest stars. In order to get some information as to the value of these photographic diameters, and to express them in star-magnitudes, comparisons were made with the measures of diameters of certain fields which Prof. Pickering and Father Hagen have standardized by photometry. The result of this comparison is now shown on the screen.

Sir R. Ball. What is the unit of the figures?

The Astronomer Royal. 0.15 of a second of arc. We have arrived at that unit because the diameters are estimated in units of $0''.3$ by each of two measurers, and, as a matter of convenience, instead of taking the mean of the two, we take the sum, so that the unit is halved. The table shows the photometric magnitude of the faintest stars shown with an exposure of six minutes on various plates, and I think on the whole they show a very satisfactory accordance. Stars beyond the 11th magnitude are certainly shown. There is another point to which I should like to refer. Since this Introduction was prepared a very valuable memoir by Prof. E. C. Pickering, 'The Distribution of Stars,' has appeared, in which he discusses the rate of increase in the number of stars of successive

magnitudes. He discusses it from the photometric material available—the increase in number of stars per magnitude going down from the brighter stars. I think he reached the magnitude $13\frac{1}{2}$, and he finds that the logarithm of the factor showing the increase in number per magnitude for the brighter stars—naked-eye stars—is 0.5, but that it falls off to something like 0.3 for the fainter stars. Now it is very satisfactory to find that the results which we have obtained from the discussion of these photographic magnitudes—quite independently—lead to results which agree remarkably with the results obtained by Prof. Pickering photometrically. In a former paper the counts of the stars have been given, and these numbers discussed, and now, from this discussion, we are able to assign the corresponding photometric magnitudes.

The President. I think the Astronomer Royal has succeeded very well in putting before us the points of general interest in this somewhat technical paper. Perhaps Mr. Hollis would like to add something.

Mr. Hollis. There is little to add to what the Astronomer Royal has said, but I may say that I am glad to think we have got our first volume out, although we must confess that we are not the first to do so; but there is a good deal of information in ours besides the mere catalogue, as you may judge from what the Astronomer Royal has explained. Potsdam has already produced three volumes, and Paris and the French colonial observatories have begun publication; Helsingfors has recently issued a part; and there is some little satisfaction in thinking that this volume covers half of our section.

Mr. Hinks. Perhaps I might express the pleasure with which we have listened to the extremely interesting statement the Astronomer Royal has given of the various matters in the Introduction to the first volume of the Astrographic Catalogue. I may perhaps be pardoned for thinking that the enormous experience that has been obtained at Greenwich seems fully to justify the view that some of us ventured to take, on more or less *à priori* grounds, of the proposals of M. Lœwy, who contended for elaborate treatment of individual plates. It now seems to be the result of most people's experience that one ought to work with as many plates as possible, treat each plate separately, and not spend too much time upon it, and take mean results from many plates, because an individual plate is not an accurate representation of the sky. The greatest difficulty is not the question of measurement, but the peculiarity of the plate, and this seems to be about the most interesting thing that the Astronomer Royal has brought out from these interesting discussions.

The President. If there are no other remarks, I am sure you will all return your thanks to the Astronomer Royal for his very clear exposition of the Introduction to the Greenwich Astrographic Catalogue. The point Mr. Hinks brought out, that the plate is responsible—if I understood the figures rightly—for nearly twice

as much of the probable error as the measurer of a particular plate, is a very important one, and warns us that we can spend too much time on a single plate, as Mr. Hinks has already said. Another thing that struck me, in the account given by the *Astronomer Royal*, was as to the great increase in sensitiveness of the plates; if that is the interpretation of the fact, we are now getting $12\frac{1}{2}$ mag. with six minutes exposure, whereas at the outset of the work it was considered it would be well if we got the 11th. We may remark also the uncertainty whether we shall get the 11th or the 13th. I think we have been fortunate in that certain well-meant attempts to fix a definite limiting magnitude within 0.1 or 0.2 were not more successful in delaying the work, and we may congratulate ourselves on the fact that common-sense had its way. The work was started rather in the dark perhaps, but with very successful results.

Mr. H. Furner, being called on by the President to read the paper presented by himself and *Mr. J. Storey*, said that the paper was a comparison of the absolute proper motions of certain double stars derived from meridian observations with the relative motions as shown by micrometer measures. *Mr. Lewis*, in his work on double stars, had found a number of stars showing large relative motion, which he suspected was due to proper motion of one or other of the components. The authors of the paper had examined more than 100 of these stars, and had reduced all available catalogue-places to 1900.0, applying Prof. Auwers's systematic corrections. For the present paper they had confined themselves to those stars which showed a relative motion of not less than $0''.1$ per year. On comparing the results with the micrometer measures it was found that about half the stars show that the fainter star has the larger proper motion. This at first caused some uneasiness, but on consideration it is what might have been expected, for if the brighter star had the proper motion it would no doubt have been detected before. He and *Mr. Storey* hoped to lay the rest of their work before the Society at a future date.

The Astronomer Royal. I am very glad that *Mr. Furner* and *Mr. Storey* have taken up this matter, for it is of much interest to determine, in the case of large relative motion, in which component the real motion is. This has evidently involved a good deal of work and is very interesting.

Mr. Crommelin. I should like to ask whether it is certain that all the stars they have studied have no physical connection, whether the proper motions of the stars are really quite independent, because that is often a difficult point to decide from the observations. A very elongated orbit might show a close approach to uniform rectilinear motion for a considerable period. If the stars were physically connected, the smaller would naturally move more rapidly, so that might be some explanation of this unexpected phenomenon. I have not seen the paper, so I do not know how far that point has been investigated.

Mr. Lewis. The purpose of this paper is not quite what Mr. Crommelin appears to suppose. It is a paper on proper motions as deduced from meridian observations. It is not certain that the stars are not physically connected, and it is this point which, incidentally, the work of Messrs. Furner and Storey will help to decide. Having shown which star is in motion, they leave the discussion of this motion to those engaged in double-star work. If in any pair of stars there is a large relative motion, and from the micrometer measures we are unable to decide which star is responsible, the aid of meridian observations is sought. Messrs. Furner and Storey have spent much labour searching for such observations, and have shown what motion, if any, the principal star has. It happens that in a number of cases the principal star shows no motion, and hence it must be the small star which is responsible. The question of elongated orbits does not enter.

The President asked Mr. Maunder if he could give a short summary of the paper presented by Mrs. Maunder and himself, although the time at disposal was somewhat limited.

Mr. Maunder said that the paper which he had to submit to the Meeting was in rather strong contrast to those which they had been considering, for instead of dealing with the most recent advances in theory and observation, it went back to the astronomy of 4000 or 5000 years ago. Mrs. Maunder and he had been led to write the paper from noticing the very loose way in which the effect of precession was referred to in books and articles dealing with Assyriological discoveries. Thus 2500 B.C. was often given as the date when the Sun at the spring equinox ceased to be in Taurus, and was recognized as in Aries. It had even been stated that the Sun was at the First Point of Aries at the spring equinox in 2540 B.C., whereas it had that position only about 150 B.C. Of course these dates assumed that the ancient Accadians and Assyrians considered the Sun as amongst the stars with which it was in conjunction. But it was possible that they associated the Sun with the stars with which it was in opposition, or by means either of the heliacal risings or settings of the stars. None of these four methods appears to have been the primitive one. But a fifth was in use at a very early age. It consisted in watching the position of the new Moon when first seen at the beginning of the year relative to a certain bright star. This star seems to have been Capella. It followed that the beginning of the second month was similarly marked by the seleniacal setting of Castor and Pollux. So long as this method was in use Taurus must have been the constellation associated with the first month of the year, and Gemini with the second. In accordance with this relation the signs for the patron deities of the first two months were a crescent moon "on its back" for the first month, and a pair of stars, for Istar and Tammuz, the "Heavenly Twins," for the second. The crescent Moon is of course more completely "on its back" when setting at the spring equinox than at any other time of the year.

(Mr. Maunder showed some slides of boundary stones, dedicatory tablets, and signets, showing the three symbols alluded to.)

Mr. Maunder continued that the second period of ancient astronomy, when Aries was taken as the first sign of the zodiac, probably arose during the period of great intellectual activity in Assyria associated with the reign of Assur-bani-pal. Hamal, the chief star of Aries, was in conjunction with the Sun at the spring equinox B.C. 700, and the equinox was observed directly by means of some mechanical method for determining time. The planets were observed in this age, and the various statements made about the god Marduk as the planet Jupiter showed that the value of that planet had been recognized as a means of determining the point of the sky in opposition to the Sun, and that the motion of that planet through a twelfth part of the ecliptic in the course of a year, and its division of that arc into three equal parts by its retrogression through 10 degrees, had also been noted.

The following papers were announced and partly read:—

S. S. Hough. "On the Determination of the Division Errors of a Graduated Circle."

E. W. Brown. "On the Degree of Accuracy of the new Lunar Theory."

E. Nevill. "On the Comparison between the Purely Theoretical and Observed Places of the Moon."

A. Stanley Williams. "On the Relative Efficiency of Different Methods of Determining Longitudes on Jupiter."

J. H. Metcalf. "Positions and Photographic Magnitudes of 90 Stars surrounding the Variable R Cygni."

H. H. Turner. "Note on the Instrumental Errors affecting Observations of the Moon."

A. M. W. Downing. "Comparisons of the Geocentric Places of the Sun and Major Planets calculated from the Tables of the American Ephemeris Office, with their Places calculated from Le Verrier's Tables for 1906."

S. A. Saunder. "Note on the Drawings of the Mare Serenitatis by John Russell, R.A."

E. W. Maunder and A. S. D. Maunder. "Note on the Date of the Passage of the Vernal Equinox from Taurus into Aries."

Royal Observatory, Greenwich. "Note on the Determinations of Positions and Magnitudes of Stars in the Greenwich Astrophysical Catalogue."

H. Furner and J. Storey. "On the Absolute Proper Motions of certain Double Stars showing large Relative Motion."

A. C. D. Crommelin. "Ephemeris for Physical Observations of Mars, 1904-6."

P. H. Cowell. "Methods of Analysis of Moon's Errors, and some Results."

The following gentleman was elected a Fellow of the Society :—

Percy Morris, Holmwood, Camborne Road, Sutton.

The following Candidates were proposed for election as Fellows of the Society :—

Zia Uddin Ahmad, B.A., Isaac Newton Student, Cambridge University (proposed by E. T. Whittaker).

Daniel Buckney, 61 Strand (proposed by W. H. M. Christie).

Robert James Wallace, Yerkes Observatory (proposed by S. W. Burnham).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1904 February 27.

President: S. A. SAUNDER, M.A., F.R.A.S., in the Chair.

Secretaries: A. C. D. CROMMELIN, B.A., F.R.A.S.
J. G. PETRIE, F.R.A.S.

Mr. J. G. Petrie read the Minutes of the last Meeting, which were confirmed.

Mr. A. C. D. Crommelin read the list of presents received, and the thanks of the Meeting were accorded to the donors.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of five new members was unanimously confirmed.

Mr. W. T. Lynn read a paper on "Occultations of Aldebaran by the Moon." He remarked that such an occultation was taking place while their Meeting was being held, but as others would take place during the next few months, he wished to draw attention to two remarkable instances of the apparent projection of this star on and within the Moon's disc—the first by Smyth and Lee on October 29, 1829, and the second by Sir Thomas Maclear on October 23, 1831. The latter suggested that the appearance was caused by the difference of refrangibility of the Moon's rays and those proceeding from the star, the star's red light being less refracted than the Moon's. Sir John Herschel suggested that the phenomenon was real, and that it was possible that the star was seen through an actual fissure in the Moon. It was a point worthy of careful observation.

Mr. Crommelin said it was unfortunate that the phenomenon should take place during the time of their Meeting, but there would be other opportunities of observing it during the year.

Mr. Lynn believed that the others occurred in the daytime.

Mr. Crommelin. Still they ought to be visible in daylight.

The President hoped members would take Mr. Lynn's hint and look out for any abnormal phenomena.

Mr. Ellis read some "Notes on the Great Nebula in Orion," and some accompanying slides were shown. He said that some authorities claimed that visible changes had undoubtedly taken place in the Orion Nebula. M. Guillemin in his book, 'The Heavens' (Sir Norman Lockyer's translation), said: "It has now been placed beyond all doubt that changes of form have taken place in the course of our most modern observations"; and, again, in a footnote: "Otto Struve, Father Secchi, and many observers in this country have completely established the fact that a change of form has taken place." In the 'Story of the Heavens' Sir Robert Ball records: "It has been demonstrated that in parts of the mighty nebula certain changes are in progress, though probably much more time must elapse before their full import can be understood." These assertions were probably based on Sir W. Herschel's opinion that the nebula underwent great changes between 1774 and 1811. So far as the writer's own observations went, a careful comparison of Dr. Common's photographs taken in 1883 with those of Dr. Roberts, of various more recent dates, gave no evidence of change of form in the last twenty years. Neither was he able to detect any variations of brightness. For the purpose of such comparison paper copies were very unreliable. It was very difficult to tell a real difference of light from an effect produced by the method of reproduction.

Mr. Wesley thought they must all agree with Mr. Ellis that all arguments in favour of change in the nebula of Orion founded upon drawings were fallacious. Holden's monograph was finished about the time that Draper made his first photographs. Holden's argument in favour of change was based on drawings alone. When they considered the extreme difficulty of making drawings of such an object, it was not surprising that they could not be trusted as evidence for change. Unfortunately, they had only had photographs for twenty years, during which time the nebula had apparently remained quite unchanged.

Mr. C. Thwaites said he had studied the Orion nebula for a great many years, and had come to the conclusion that, as far as the appearance to the eye was concerned, there had been no change. Any slight variation that appeared in photographs, he believed, was solely due to differences of the apertures of the telescopes used or to halation, which would be intensified by long exposures.

Mr. Holmes said that as he had so often objected to any conclusions being based on drawings, it was satisfactory to him to hear others coming round to that opinion.

The President said they must bear in mind that twenty years was an exceedingly short period in the life of a nebula. He congratulated Mr. Ellis on the series of beautiful pictures he was producing with his instrument.

Mr. Crommelin read a paper by *Mr. W. F. Denning* entitled "Dark Spot in Jupiter's South Temperate Region," illustrated by two slides. The writer said he first drew attention to this remarkable object in the *British Astronomical Association's 'Journal'* (January 21, 1902). The spot continued visible at the present time, and exhibited no signs of approaching disappearance. Between 1901 June and 1904 January, its mean period of rotation was $9^h 55^m 18^s.7$, and when passing the great red spot in July 1902 it was about 87° long, but in 1903 its mean length was $48^\circ.3$, or only about half the length.

Mr. Goodacre remarked that it seemed somewhat a misnomer to call this a spot. A marking between 80° and 90° in length appeared to be not a spot, but a new belt or a new dark region altogether, especially as 90° is equivalent to a length of 60,000 miles.

Mr. Maunder then gave an account of a memoir just published by Prof. George E. Hale and Mr. Ferdinand Ellerman of their work with the Rumford Spectroheliograph of the Yerkes Observatory, illustrated by a number of slides.

Mr. Geo. M. Seabroke said he had been much struck with the beautiful photographs of Prof. Hale, who had his warmest congratulations. A little difficulty, however, occurred to him in the interpretation of the results, that perhaps Mr. Maunder could explain. When a photograph of the calcium vapour was taken on the central portion of the line where it was bright, they would expect to get an image of the bright calcium cloud. But when the slit was shifted to the side it was taking in the *absorption* effect of calcium vapour, and one would expect, at first sight at least, to get the vapour represented as dark clouds on a bright background.

Mr. Crommelin said there was a point upon which he would ask Mr. Maunder whether he could throw any light; he alluded to the very characteristically different shapes of the flocculi that they got in the hydrogen light from those in the calcium light. Those obtained by the calcium light were rounded and woolly-shaped, like cumulus clouds. Those obtained by the hydrogen light were feathery, and suggested the shapes of leaves and ferns seen on a frosted window. It seemed to him one of the mysteries of the solar atmosphere how the hydrogen gas could be distributed in these peculiar forms. It seemed to suggest some law analogous to crystallization, but applying to gas instead of to solids.

Mr. Newbegin said that Mr. Hale, in his beautiful researches, seemed to be going lower and lower down into the secrets of the Sun. He presumed that, although the gases were all mixed up, yet by taking one particular line at a time they were able to deal with that gas alone.

Mr. Maunder said he would endeavour to reply to the questions that had been asked. For the sake of illustration, they might consider the solar atmosphere as consisting of, say, four different

strata of glowing calcium gas, the lowest stratum being necessarily the most dense, the uppermost the most rare. Considered in this way, the densest stratum would give a very broad dark K-line of calcium. If there were a region of the Sun where only the second stratum existed, it would give a less broad line, whilst a region where the calcium might be supposed to be very rare would give a very narrow dark K-line. Actually, however, the strata lay one above the other, but it was clear that if the second slit of the spectroheliograph were placed in such a position in the spectrum that it fell outside the dark K-line of the second stratum, but within the dark line of the first stratum, then the photograph which would be obtained would be a photograph given by the light of the calcium gas in the lowest of the four strata. The first slit of the spectroheliograph, travelling over the image of the Sun, showed the distribution of these localities of relatively brighter or darker calcium vapour over the entire disc. The other strata might be similarly dealt with. The light received from any one stratum examined would be chiefly from its upper surface, and therefore, at each particular setting of the second slit, we were practically dealing with the calcium light at one particular level alone; or, rather, with the integration of the calcium vapours at the various levels comprised within the width of the slit, for the slit necessarily bore a considerable proportion to the breadth of the K-line.

A paper on "Vibration" was read by *Mrs. Herkless*.

The President, in thanking *Mrs. Herkless* for bringing such an extremely interesting and important subject before the Association, said that a very great amount of research must have been required in the preparation of the paper.

Mr. Crommelin read a paper by *Mr. J. E. Gore* on "The Companion of Algol."

Remarking on the paper, *Mr. Crommelin* said that in the Sirian system we had a system with a spectrum not very unlike that of Algol, which had a visible companion which was about 10 magnitudes fainter than the bright star, and yet was about half the mass, so that there seemed nothing impossible, as *Mr. Gore* seemed to imagine, in the companion being half the mass of the bright star and yet giving out only a small fraction of its light; they had certainly that state of things in the Sirian system.

Mr. Gavin J. Burns read two papers:—(1) on "The Zodiacal Light"; and (2) on "The Magnitude of α Orionis."

In the first paper *Mr. Burns* urged members of the Association to look out for the Zodiacal Light on all suitable occasions this spring. The necessary conditions were that there should be neither moonlight nor twilight. These conditions would be fulfilled on the following dates:—

From March 5, at 7.40 P.M., to March 18, at 8.10 P.M.

From April 2, at 8.30 P.M., to April 17, at 9.0 P.M.

In the second paper Mr. Burns described the exposing of plates so as to obtain trails of α and λ Orionis, from comparison of which he deduced that after brightening up to the extent of a magnitude some time in 1902, α had gone back to its normal condition.

In reply to an inquiry *Mr. Burns* said the best time for observing the zodiacal light was, as nearly as possible, two hours after sunset.

The President thanked *Mr. Burns* for bringing forward the subject, and the Meeting adjourned.

ROYAL METEOROLOGICAL SOCIETY.

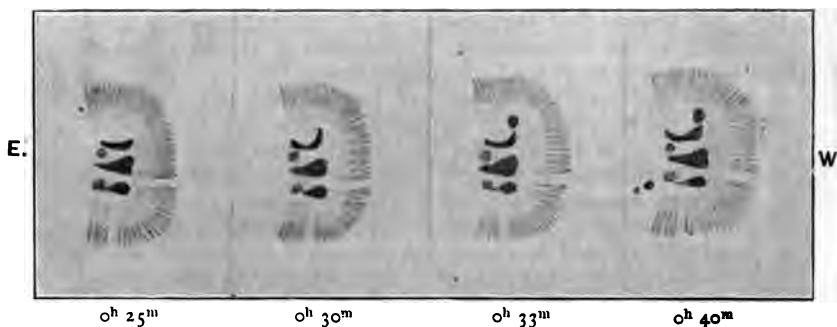
THE monthly Meeting of this Society was held on the evening of Wednesday, March 16, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker*, President, in the Chair.

Mr. Richard H. Curtis delivered a lecture on "Water-Vapour," which he illustrated by experiments and lantern-slides. After explaining that water-vapour is water in the state of a gas, he proceeded to speak of the processes of evaporation and condensation, and showed that the capacity of the air for moisture varies according to the temperature. The amount of vapour in the air has a great deal to do with our personal comfort. A dry cold air is not so unpleasant, and does not feel so cold, as a damp air, although the temperature of the damp air, as shown by the thermometer, may be higher than that of the drier one. But our sensations are often at variance with the thermometer, and this generally so, because of the way in which they are affected by the vapour of the air. If the air be dry, a degree of heat can be enjoyed which would be simply unendurable if it occurred with a humid atmosphere. The amount of vapour in the air is ascertained by means of an instrument known as a hygrometer, various patterns of which were shown by the lecturer. He then referred to some phenomena of the atmosphere in which water-vapour plays a leading part, and described the formation of dew, hoar-frost, fog, cloud, halos, rain, snow, and hail. The lecturer concluded by saying that he had tried to show how a particle of water may be taken from the ocean and stored away invisible in the atmosphere above it; how that particle may travel for many miles to distant parts of the globe, and then by the action of another of Nature's processes be changed back again into water, and fall once more upon the ground beneath. There it may unite itself to other similar drops, and together form a tiny stream, which may gradually grow to the dimensions of a river; and once again our drop of water may find itself a constituent part of the ocean, to repeat its pilgrimage, and carry on the part it is destined to play in the economy of Nature.

Sun-spots.

JUST before noon on January 22 I observed, with a 4-in. Cooke refractor, a very extended group of 4 pretty large sun-spots in the N.W. quadrant of the disc. I drew the largest member of the cluster, the umbra of which was crossed by some faint, hair-like veins of faculæ. The uncovered portion looked almost black and formed what is usually called the nucleus. But the lighter umbra of a spot appears to be simply the nucleus partially veiled with overlying films of luminous material.

Having finished the drawing of the largest spot of the group, I took a sketch of the preceding member (which contained a conspicuous triple umbra and showed an absence of penumbra on its east side) and immediately afterwards noticed some remarkable changes materially affecting its aspect. The changes occurred rapidly under my eyes and were quite unmistakable. I made rough delineations of them at short intervals. One of the three umbræ alluded to was chiefly concerned; a small spot also disappeared, while at 0^h 40^m a pair of little spots not seen at 0^h 33^m had formed. The whole object seemed to be in a state of agitation.



Changes in a Sun-spot, 1904, January 22.

During occasional solar observation since 1865 I have never previously remarked such rapid alterations in detail. Some of these were perceptible in a very few seconds and could be distinctly traced in their operation. Unsteady definition had nothing to do with the phenomena observed; the image was, in fact, pretty good, and the rippling not so strong as usual.

Having lately made a careful study of several large sun-spots, with various powers up to 240, I conclude that the supposed "willow-leaf" structure, as shown in Sir J. Herschel's 'Outlines' on the basis of Nasmyth's drawings, does not quite correctly express the shape of the luminous filaments forming the Sun's outer envelope. The objects are rounded at the ends and the term "rice-grains" appears to be more applicable.

Many sun-spots are drawn with the penumbral details radiating symmetrically from the central umbra, but this is often far from being accurate. The penumbra includes forms both light and dark lying at all angles relatively to the middle portion, though in some large, compact spots the general effect is that of fibrous material diverging pretty consistently from the umbra.

Whether or not the spots are cavities is still perhaps an open question. It is certain that if the crateriform structure affords the true explanation then the interiors are exceedingly shallow. The changes witnessed in the spot of January 22 gave me the impression of being produced by running lava evolved from underneath the luminous material forming the outer envelope.

Some years ago the telescopic aspect of the Sun occasioned a large amount of attention and discussion, and a number of interesting papers appeared on the subject from Nasmyth, Dawes, Sir John Herschel, Secchi, Lockyer, Stone, De la Rue, Fletcher, Brodie, and others (see 'Monthly Notices,' vols. xxiv. & xxv., and 'Astronomical Register,' vols. ii. & iii., 1864 & 1865). Since that period (about 40 years ago) the study of solar phenomena has scarcely been continued with the same energy and success and our knowledge of visible details appears to have shown little material advancement. The spots have been regularly observed, it is true, their variable numbers recorded and new evidence relating to the periodicity obtained. But with regard to their structure in detail, their changes of appearance, their formation and dissolution, their possibly cyclonic attributes, their recurrence in same parts of disc, &c., &c., not much new light has been thrown in recent years. Yet the study of sun-spots offers a very promising field for further research and new discoveries. It is scarcely sufficient to obtain photographs or to telescopically view the Sun for half an hour or so and draw the spots in the usual way. It seems more necessary to success that the Sun should be watched almost continuously on clear days for say 6 or 7 hours in winter and 12 or 13 hours in summer and the details of spots traced with critical care through their various developments. It would not be essential, during such protracted observations, to follow the behaviour of all the spots visible, but rather to select a particular spot or part of a spot and, with high powers, keep that closely under inspection, making drawings and notes at short intervals. In this manner interesting features might certainly be detected which have been enabled to escape the brief and hurried observations usually obtained. The majority of observers turn their telescopes on the Sun and temporarily engage in measuring, drawing, or fixing the positions of the spots. This done the observation immediately terminates. But it is certain that the solution of the problem affecting certain peculiarities of structure and variation in sun-spots is to be sought rather in long-continued observation, and it is hoped that some observers having the necessary time and instruments will take up the proposed work. Quite possibly the system of watching for

lengthy periods has been adopted before, as it must naturally have suggested itself as very desirable; but if so, it does not appear to have furnished the successful issues which it is so well calculated to bring about.

The plan of *direct observation* is of course vastly preferable in critical work on the Sun to any other method of viewing the spots. It is very convenient and useful in some respects to throw the Sun's image on to a white surface, and fair general views of the spots are obtainable in this way, but the plan utterly fails when we come to the study of the really difficult and delicate features of the solar disc.

Bishopston, Bristol,
1904, March 18.

W. F. DENNING.

The Rumford Spectroheliograph of the Yerkes Observatory.

THE discovery by Prof. Hale in 1891 of the reversal of the centres of the dark lines H and K in scattered regions over the surface of the Sun's disk, as well as in the prominences, and his subsequent work at the Kenwood Observatory with the beautiful instrument he designed for recording these phenomena, are still fresh in the minds of astronomers. Of the recent developments at the Yerkes Observatory we now have (in vol. iii. of the 'Publications' of that Institution) a most interesting account, illustrated with a splendid series of half-tone plates.

The Rumford Spectroheliograph is designed to photograph, in monochromatic light, the seven-inch solar image at the focus of the 40-inch refractor, and its parts have of necessity to be built on a scale corresponding to that of the great telescope. The financial question naturally offered a serious obstacle to the construction of such an instrument at the outset, but this was finally disposed of by a grant from the Rumford Fund. Prof. Hale then proceeded to grapple with the many problems presented in designing an instrument which should be capable of dealing satisfactorily with so gigantic a primary image, and his account of the way in which the very formidable difficulties encountered have been faced and finally overcome affords exceedingly instructive and interesting reading.

The principle by which the spectroheliograph builds up a monochromatic image of the Sun with light from any selected line in the spectrum is very simple, and may be briefly described as follows:—When an image of the Sun is thrown on the slit plate of a spectro-scope, the dark lines in the spectrum represent monochromatic images of a narrow section of the solar surface defined by the slit, and the lines belonging to the different elements may give different versions, so to speak, of this same section of the Sun. The function of the spectroheliograph is to isolate, by means of a second slit, any one of these monochromatic images and to build up with a number

of such sections a complete image of the disk. This is effected by imparting a continuous movement to the first slit relative to the primary solar image, and by a simultaneous movement of the second slit relative to a photographic plate placed behind it.

It is obviously immaterial whether the slits themselves are made to move or the solar image and photographic plate are moved, the only essential condition being that the selected spectrum-line falls within the second slit and on to the plate throughout the exposure.

In the Rumford Spectroheliograph the slits are stationary, and the necessary movement of the image across the first slit, which is arranged to be parallel to the equator, is effected by the declination motor of the equatorial, a corresponding movement being imparted to the photographic plate behind the second slit.

No great dispersive power is needed for work with the very broad dark lines of the solar spectrum, and two simple prisms are found to be sufficient; these are, however, supplemented by a powerful grating for work with the finer lines. The method of correcting for the distortion due to the curvature of the lines of the prismatic spectrum (suggested by Prof. Wadsworth) is so simple and satisfactory that this drawback to the use of ordinary prisms in spectroheliograph design need no longer be considered.

Foremost among the rather numerous and exacting adjustments demanded by the spectroheliograph is the one whereby the second slit is made to coincide exactly and throughout its length with any selected line in the spectrum. This is not a very difficult matter if a sharply-defined line is chosen in a region of the spectrum that is easily visible to the eye. But in the case of the lines H and K, which give such interesting and beautiful images of the Sun's disk, as well as the prominences at the limb, the problem is a much more difficult one: these lines are at the limit of the visible spectrum and, especially K, are not at all easy to see; moreover they are broad indefinite bands, in which the slit must be set exactly at the centre where the bright reversals occur of those forms which Prof. Hale has appropriately named the *focculi*.

If the slit is set to one side of the centre the picture obtained is much less brilliant in its contrasts, and the prominences are not shown at all. Interesting and very instructive results are nevertheless obtained with the slit in this position on the H or K bands, and Prof. Hale has made a special study and comparison of the photographs obtained with the slit set at the centre and at various points in the shading on either side.

The H and K lines in the solar spectrum consist of three distinct parts: first, a broad hazy band representing the absorption of the lowest and densest calcium vapour lying immediately over the mean photospheric level, but appreciably below and between the highest summits of the faculæ. This Prof. Hale designates H₁ and K₁. Next comes an interrupted bright line superposed on the centres of the bands, which he calls H₂ and K₂, representing the emission of the relatively hot calcium vapour rising immediately

above the ordinary faculæ. Finally, there is a narrow dark line, H_2 and K_3 , superposed upon the broader bright line, which is thereby mostly divided into two parts. This represents the absorption of the highest and relatively the coolest region of the chromosphere.

It would seem to be possible, therefore, by setting the second slit of the spectroheliograph successively upon the three different portions of, say, the K line, to obtain calcium images representing three different altitudes above the photosphere. It is, however, scarcely possible, without very great dispersion, to distinguish between the central absorption line and the underlying bright line, and when the slit is placed as near as possible to the centre the result is a photograph made up of light from the intermediate K_2 level, modified to some extent by the varying absorption of K_3 .

But in the case of the broad K_1 band the slit can be set in more than one position which will be quite clear of the central K_2 line. When placed entirely outside the band upon the continuous spectrum, the image is the same as an ordinary direct photograph, showing the faculæ increasing in contrast towards the limb. But with the slit just within the borders of K, Prof. Hale considers that he obtains images of the very lowest level of calcium vapour. Farther within the band a higher level of calcium is reached, yet not so high as the K_2 level obtained with the slit central.

In discussing the results obtained in this way, Prof. Hale adopts this view as a "working hypothesis," namely, that the calcium flocculi are depicted sectionally in at least three different levels above the photosphere, and the evidence afforded by the photographs certainly seems to bear out this idea. A serious objection, however, would seem to follow from the fact that in photographs of the spectrum itself the true reversals of H and K seem to be entirely confined to the central H_2 and K_2 region of the lines. Only in the rare cases of violent eruptions are there ever any signs of lateral spreading of the bright lines; and the shading on each side appears almost always to be uniformly dark, except only where it is crossed by the faintly bright continuous bands of the true faculæ. These bands, however, sometimes give a deceptive appearance of true reversal of the shading, as is well seen in plate ii. fig. 2.

An alternative hypothesis would be that the K_1 images really represent the true faculæ, and are photographed by the continuous spectrum superposed upon the K band. On this view they cannot be regarded as low-level calcium vapour. It is possible, however, that true calcium emission may become evident where shown up upon the dark background of a spot. The interesting comparisons given on plates v. and vi. would have been more instructive on this point had the region photographed been situated near to the limb, where the faculæ proper would have shown up in much greater contrast.

The increase in brightness of the flocculi with the slit set nearer

the centre of the band, but still outside K_1 , might be explained as an effect of contrast only. It is to be borne in mind, however, that in this position, so near to K_2 , there is the possibility of partial reflection of the bright K_2 from one of the (highly polished) jaws of the slit itself.

Some of the most interesting plates are those showing the solar surface as depicted by the hydrogen line $H\beta$. This is a return to a very early phase of spectroheliograph work. In 1891 the present writer attempted, without success, to obtain photographs of the prominences projected upon the disk with a simple form of spectroheliograph, making use of the $H\beta$ line, with which it was not difficult to photograph them at the limb. The reason of this failure is now apparent: the ordinary prominences are less bright than the chromosphere from which they rise; consequently, if they are to be seen at all projected on the disk it is probable they would appear as *dark* forms, the cooler hydrogen in the prominence absorbing the light from the hotter region below. This appears actually to be the case; the photographs obtained by Prof. Hale and Mr. Ellerman with $H\beta$ show very curious dark forms distributed over the disk, and evidently due to absorption effects. Some of these seem to correspond in position with the prominences, although this relation is not made out with certainty. But there is a very evident correspondence in position of the larger dark masses with the bright calcium flocculi, and very interesting are the comparisons given of the $H\beta$ images and the calcium images in the neighbourhood of spots. These beautiful plates well illustrate the great possibilities of the spectroheliograph in extending knowledge of the intimate structure of the solar surface and of spots; for not only hydrogen and calcium, but many other elements having fairly strong absorption lines may be made to yield their own special versions of the interesting region lying immediately above the photosphere.

In concluding his discussion of the photographs, Prof. Hale draws attention to the fact that his results, so far as they go, are in accordance with the views expressed by the writer in attempting to explain the predominance of the enhanced lines in the flash-spectrum. In this connection it would be of great interest to compare the image given by any normal and isolated iron line with that yielded by an enhanced iron line. Enormous dispersion would, however, be needed to deal effectively with one of the latter lines, of which perhaps the line at $\lambda 4584.0$ would be the most suitable.

But in this, as in many other lines of investigation for which the spectroheliograph is so well adapted, the persistent effort of many observers is imperatively needed, and it is to be hoped that Prof. Hale's appeal for cooperation in this work will meet with a wide response. The magnificent series of photographs of the great southern spot of October last should whet the appetite for further research, as they certainly emphasize our total ignorance of the real nature of sun-spots.

J. EVERSHED.

CORRESPONDENCE.

*To the Editors of 'The Observatory.'**Peter Apian.*

GENTLEMEN,—

All astronomers are familiar with the name of Peter Apian, because it was by a comparison of his observations in 1531 and Kepler's in 1607 with those of himself and others in 1682 that Halley was able to establish the periodicity of the comet which afterwards was called by his name, and which the calculations of Pontécoulant lead us to expect will again return to perihelion in the month of May 1910. But not much else is generally known about Apian. Dr. Siegmund Günther, however, wrote a short account of him and of his son Philipp, which appears in the Transactions of the Bohemian Gesellschaft der Wissenschaften, and was published at Prague in 1882. His real name is commonly stated to have been Bienewitz, but Günther says that it cannot be decided whether the correct spelling is that or Bennewitz; that it was Latinified into Apianus must surely render the former the more probable. The family belonged to the town of Leisnig, which is situated nearly halfway between Leipzig and Dresden. All that is known of the astronomer's parents is that their names were Martin and Gertrud. Peter was born in 1495. Until 1518 he was educated at Rochlitz and Meissen, but in that year he proceeded to Leipzig, where he probably adopted the name Apianus, which he always spelt, it may be remarked, with one p. Later, he went to Vienna, where he became acquainted with Peurbach, Müller (commonly called Regiomontanus), and others. Afterwards he settled at Landshut, in Bavaria, where he married Catharina, a daughter of Thomas Mosner, a Councillor, and soon afterwards his reputation led to his being called to the chair of Mathematics at Ingoldstadt, and to his being ennobled by the Emperor Charles V. His geographical work '*Cosmographia*' appeared in 1529; his '*Astronomicon Cæsareum*' in 1540. In the latter are given his cometary observations, including the discovery of the comet of 1531, which it would here be an anachronism to call Halley's. It was in that year that his second son, Philipp, was born, who succeeded his father in the professorship at Ingoldstadt on the death of the latter in 1552, in the fifty-eighth year of his age.

It is not desirable here to touch upon Apian's astrological and other speculations; but it may be mentioned that he was the first to notice that the tails of comets are always pointed nearly in the direction opposite to that of the Sun, which the poet Kästner has commemorated in the lines:

Hat er daran gefehlt, so hat er auch entdeckt,
Dass von der Sonne stets der Schweif sich abwärts streckt,
Und der ist wenigstens noch keines Tadel's werth,
Der uns, so oft er irrt, auch neue Wahrheit lehrt.

Philipp Apian removed from the professorship at Ingoldstadt to that at Tübingen in 1558, and died at the latter place in 1589. Like his father, his studies included those of antiquities and other subjects, besides mathematics and astronomy, but his name is not connected with any discovery.

Yours faithfully,

Blackheath, 1904, Mar. 7.

W. T. LYNN.

The Occultation of Aldebaran.

GENTLEMEN,—

On February 24 last, Aldebaran disappeared instantaneously as seen here. I employed a power of 50 on $3\frac{3}{4}$ inches. Another observer close at hand, using 70 on $2\frac{1}{4}$ inches, had the same impression. The sky was perfectly clear at the time, but I could not discern the dark edge of the Moon. In the earlier half of the last century one seldom reads of occultations of this star without the phenomenon of projection being noticed. Admiral Smyth's explanation in the second volume of the 'Cycle' may be quoted: "This phenomenon seems to be owing to the greater proportionate refrangibility of the white lunar light than that of the red light of the star, elevating her apparent disc at the time and point of contact." I have, however, seen Regulus, the reverse of a red star, apparently within the Moon's limb for three beats of the chronometer.

At the occultations of Aldebaran on January 6 and March 2, 1887, I witnessed an instantaneous disappearance each time, but each time the emersions from the bright edge of the Moon were not nearly so sudden: on the former occasion I noticed that the star "crept out leisurely from a point just north of the Mare Crisium"; on the latter it gave me the impression of being visible "on the bright edge 5 seconds." Admiral Smyth stated that there could be "no mistake" about his impression of projection on October 15, 1829, because he saw the bright line of the Moon outside the star, as also did Dr. Lee with a smaller instrument. Generally speaking, it seems to be at the bright edge that this phenomenon of projection has been noticed.

Naked-eye observations of this sort are, probably, not worth very much. But the earliest may be that of Fabricius on March 4, 1607. See the "Paralipomena" to Tycho's 'Historia Cælestis.' On this occasion it is recorded that this observer saw "applicationem lunæ corporalem ad oculum ☿ et vidit stellam nondum tectam, quando inter illam et partem lunæ lucidam, minus ipsa parte lucida interesse videbatur. . . . Inde colligit amictum esse globum lunæ sphaera ærea laxiore quæ luce Solis combitâ, se nobis pro vero ipso lunæ corpore ostendit," &c. A figure is appended showing the star just on the edge.

In 'Monthly Notices' for 1860 will be found a paper by Rev. T. Webb, pointing out an instance of projection in lunar occultations on March 7, 1794, when it was perceived by the naked eye. In

the 'Cometographie' of Pingré, tom. i. p. 321, an instance of this sort is referred to under the date of A.D. 547, when he relates that "une étoile entra sur la disque de la Lune."

Melplash Vicarage, Bridport,
1904, Mar. 5.

Faithfully yours,
S. J. JOHNSON.

GENTLEMEN,—

I saw the occultation of Aldebaran in a clear sky and with good seeing with my $4\frac{1}{2}$ O.G. refractor. It was my privilege to observe the "anomalous" phenomenon referred to in the current number of the *Observatory*. At ingress Aldebaran was distinctly hanging on the lunar limb (faintly illuminated by earth-shine) for a few seconds. I observed this carefully, and think it was a mere effect of irradiation.

I remain, Gentlemen,

Montpellier, France,
1904, Mar. 8.

Yours truly,
MARCEL MOYE.

Stellar Distribution.

GENTLEMEN,—

With reference to the question of stellar distribution and the point at which the fainter stars begin to "thin out," I have made a careful enumeration, according to magnitudes, of the stars shown on the photographic chart of the Pleiades given in Klein's Star Atlas. The photograph was taken by the Brothers Henry at Paris. The space considered includes three square degrees, and contains all the brightest stars of the Pleiades. The following are the results I have found:—

Magnitude.	Number.	Totals.	Ratio.
3	1	1	—
4	2	3	3
5	3	6	2
6	5	11	1·83
7	1	12	1·1
8	12	24	2
9	18	42	1·75
10	28	70	1·66
11	57	127	1·81
12	113	240	1·9
13	157	397	1·65
14	169	566	1·42
15	267	833	1·47
16	448	1281	1·53

As the theoretical value of the ratio is nearly 4, it will be seen that the actual value for all magnitudes is considerably less than it should be in the case of an equal distribution of the stars in space. Possibly some of the fainter stars visible in the original photograph may be missing in the reproduction, but still the defect

is apparent even in stars down to the 11th magnitude, and it seems probable that *all* the stars to this magnitude, at least, or perhaps fainter, are shown on the plate in Klein's Atlas.

The total number counted on an area of three square degrees is 1281 stars. This would give for the whole sky (41,253 square degrees), if of the same richness, a total of 17,615,031 stars.

I am, Gentlemen,
Yours faithfully,
J. ELLARD GORE.

Dublin, 1904, Mar. 7.

Antoine Darquier.

GENTLEMEN,—

The lamented death of M. Perrotin directs our thoughts to astronomy at Toulouse, where he laboured until his appointment to M. Bischoffsheim's new establishment at Nice in 1880. Although the present observatory at Toulouse dates only from 1840 (the late M. Petit having been its first director), there was a much earlier establishment there. the creation, at his own expense, of Antoine * Darquier, who was born at Toulouse in 1718. His first publication was a 'Uranographie' (at Paris) in 1771, but of this I have not been able to meet with a copy. Lalande says of it: "Il est très commode pour apprendre à connaître le ciel. On l'a réimprimé avec les *Lettres sur l'Astronomie* du même auteur." He published two volumes of observations made at Toulouse; the first of these appeared at Avignon in 1777, the other five years later. Copies of both are in the library of the Royal Society, but only of the first in that of the British Museum. With regard to them, Lalande remarks: "Jamais le zèle d'un savant éloigné de la capitale n'a été aussi soutenu et aussi utile."

Darquier describes in his preface his early devotion to astronomy and the instruments with which he commenced his observations, the first of which was of the eclipse of the Sun on the 25th of July, 1748, the magnitude of which amounted to $7\frac{1}{2}$ digits; he noted, besides the times of the beginning and end, the covering and uncovering of a large spot in the north-western part of the disc. He observed also a partial eclipse of the Moon on the 8th of August in the same year, and another eclipse of the Sun on the 8th of January, 1750, when there were five beautiful spots on the disc; at the beginning of this eclipse the Sun was covered by cloud, but the end was observed and the obscuration and reappearance of the spots. An occultation of ρ Aquarii was observed on the 21st of November, 1754, of θ Capricorni on the 18th of July, 1755, of λ Aquarii and κ Virginis on the 9th and 27th of December respectively in the same year, and of some of the stars of the Hyades on the 7th of May, 1756; also eclipses of the Moon on the 27th of March, 1755, and the 18th of May, 1761. He did not

* Poggendorff erroneously gives the Christian name as "Augustin," but Lalande must be the best authority.

fail to observe Halley's Comet in 1769, but it was not till April 14 that he learnt (from the *Gazette de France* of April 6) that it had been re-discovered on the previous Christmas-Day by Palitzsch, and seen towards the end of January at Paris by two astronomers who, says Darquier, "n'avoient eu garde de faire part de leur découverte. Cette jouissance exclusive est peut-être un grand plaisir; mais il est très possible qu'elle nous ait privé de bonnes observations dans les beaux climats de la France." One consequence of this selfish reticence (which produced the above regretful comment) was that Darquier, zealous observer as he was, did not succeed in seeing the comet until the morning of the 16th of April, somewhat more than a month after the perihelion passage. He also observed the third comet of 1759, which he first saw on the evening of the 16th of January, 1760, about a week after its discovery*. This comet approached the Earth within a comparatively short distance, and its apparent motion was very rapid.

I cannot, of course, enumerate all Darquier's observations here, but may remark that it was not until 1761 that the possession of a quadrant (made by Canivet, under the direction of de Lacaille) enabled him to determine accurately the latitude of his observatory. His first instrument was a reflecting telescope from London, by Short, 18 inches in focal length. Canivet made him a transit-instrument in 1756 †, and two years later he obtained an achromatic telescope, "singulièrement bonne," by Dollond, of $7\frac{1}{2}$ feet focal length. His measurements of positions of celestial bodies began with the acquisition of the quadrant in 1761, and a frontispiece to the volume gives a view of the author making an observation with it. It is interesting to have what appears to be a very good portrait of Darquier; as I remarked, his second volume was published in 1782, but I do not know of anything he did subsequently. His death did not take place till 1802, in the eighty-fifth year of his age, and he appears to have spent his whole life at Toulouse, mentioning only in his preface one journey to Paris in 1756, during which he procured his first transit instrument.

Darquier mentions, as a matter of just pride, that on the 26th of April, 1775, he saw Jupiter with his quadrant when following the Sun by only one hour and nine minutes; nearer the time of the planet's conjunction, he believes, than any previous observer. Probably very few subsequent ones have seen it as near.

Yours faithfully,

W. T. LYNN.

Blackheath, 1904, Mar. 11.

* Perihelion passage, December 17, 1759.

† This was long afterwards superseded by a new one obtained in March 1775.

PUBLICATIONS.

NATURAL LAW IN TERRESTRIAL PHENOMENA*.—In this handsome and well-printed volume of nearly 400 pages, profusely illustrated with charts and diagrams, Mr. Digby enthusiastically advocates the "theory" of Mr. Hugh Clements, "a life-long student of the various sciences and arts," regarding the prediction of weather and other natural phenomena, as mentioned in the title of the book. Mr. Digby takes the matter quite seriously, and asks for Mr. Clements's discoveries "the most exhaustive examination, without prejudice and without passion"; but heterodoxy has, we know, its fascinations, though here it will scarcely lead to the martyr's crown. As to the cause of weather-change, Mr. Clements, like Petruchio, seems to exclaim "It shall be moon, or star, or what I list"; and Mr. Digby, like Katharina, becomes similarly brought to say "I know it is the moon."

But let us examine the matter a little. At page 281 of the book Mr. Clements explains his discovery. Briefly it is that "to understand the incidence of weather it is necessary to indicate the agencies that control or modify its production." The prime motors are the Moon and the Sun: the Moon primarily, be it noted. As to the Sun, his influence "may be taken as a constant," since at any part of the Earth's surface it is "exactly the same on any given date of any given month, year after year, for centuries," and may in consequence "be neglected [poor old Sol!]" and the Moon's motions alone considered in the estimation of weather-changes." As to the method employed, we read, page 343, that "in order to predict the weather correctly [mark this] for any given place, it is necessary to be able to predict the actual height of the barometer." To do this for any day in the future "it is essential to know the mean height for a corresponding day in the past, together with the times of the Moon's transit, the Moon's parallax, and the declinations of the Moon and Sun, for the past as well as the predicted day." Then it is the various lunar cycles that do the trick. For the corresponding day in the past is to be one distant from the future day by a period equal to some multiple or combination of lunar cycles, such as will make up very nearly an integral number of years, so giving certain similarities in the Moon's position. The time of revolution of the line of apsides, the regression of the node, and other cycles are brought into play, the theory being that, with similar position of the Moon, the barometer stands at the same height. But since the two positions of the Moon will not be quite the same, Mr. Clements, never without resource, page 344, has determined to his satisfaction that, for the outstanding difference, a variation of 1' in horizontal parallax

* 'A Study in the Causation of Earthquakes, Volcanic Eruptions, Wind-Storms, Temperature, Rainfall; with a Record of Evidence.' By William Digby, C.I.E., Fellow of the Royal Statistical Society, Member of the Royal Asiatic Society, Author of various works on Indian Economics.

changes the barometer reading by 0.03 in., and a variation of 1° in a mysteriously compounded difference of declination changes it by 0.08 in., with something also depending on difference of time of transit. With such material the reading of the barometer on the day in the past is readily reduced to that of the day in the future. Thus the Moon determines the height of the barometer, and consequently also the weather at any given place.

Further the mystery of sun-spots is no mystery to Mr. Clements. They are "caused by planetary attraction." And since the positions of the planets can be calculated for any number of years in advance, so can sun-spots. But enough. The book contains innumerable quotations from the writings of noted physicists to the effect that practically the Moon does not influence weather, and that to the Sun are the weather-changes mainly due; but these are all gaily swept aside. It is the Clements theory that holds the field. It is asked, page 306, "Why should not the British Government make use of the present opportunity to rightly equip the Forecasting Branch of the Meteorological Office, furnish it with an adequate staff of computators, and thus constitute it one of the most valuable assets of an Empire, three fourths of whose populations depend upon agriculture, that is upon the weather, for their daily bread." Finally, page 307, it is hoped "that the tools will be given to the man who alone among the many millions of British subjects has proved himself capable of using them."

The many failures to prove that any definite relation exists between the Moon and weather makes the publication of the present monumental exhibition of misdirected energy sufficiently remarkable, but in these matters confidence in a result seems to be in inverse ratio to the value thereof. W. E.

MEASURES OF DOUBLE STARS. (S. W. BURNHAM.)—This latest contribution of Prof. Burnham's is from vol. viii. of the First Series of 'Decennial Publications of the University of Chicago.' In making the working list Prof. Burnham's endeavour was to include no star likely to be observed elsewhere, and so obtain some notion of what the neglected pairs had been doing. Naturally the majority of such stars are from the catalogues of the Herschels and South, and the rejected stars of Struve. The present set of about fifteen hundred measures is a first instalment of a heavy piece of work. No endeavour was made to discover new pairs, but nevertheless 18 pairs have been added, which brings the Burnham numbers to 1308. Some of these will no doubt prove of great interest, such for example as β 1295. The principal star is 2 Camelopardi, which with a companion $1''.5$ distant was known as Σ 566. Prof. Burnham now makes the principal star itself a close double. As the proper motion is $0''.1$ per annum, this is a fine triple system. Looking at these new discoveries one cannot but feel that possessors of small telescopes have much to answer for. Here are 18 good pairs, of which 90 p. c. will doubtless prove

binary, found without search. Had the 40 ft. telescope been free this number might very well have been 200. But if the wide pairs are neglected by small instruments they must be measured with the large instruments. This requires some courage.

NOTES.

COMET NOTES.—Mr. F. E. Seagrave gives a search ephemeris for Encke's Comet in *Popular Astronomy* for February. Perturbations have not been allowed for, so the errors may be considerable.

Nearest Earth 1904 Nov. 23; distance 0.374. Nearest Sun 1905 Jan. 4; distance 0.342. Brightest 1904 Dec. 26.

Ten years ago, when the conditions were similar, the comet was a very brilliant telescopic object, and faintly visible to the naked eye; it is therefore likely to be an easy object for small telescopes next November and early in December, after which it will pass too near the Sun for observation.

Ast. Nach. 3934 contains a definitive orbit of Comet 1898 X. by S. Scharbe, of Dorpat.

This comet was only under observation one month, during October and November 1898. Planetary perturbations have been applied, though very minute. The following elliptical elements are adopted:—

Epoch of osculation	1898 Nov. 5.0.
T	1898 Nov. 23.19512 Berlin M.T.
ω	123° 32' 24" } 1898.0.
Ω	96 18 12 }
i	140 20 57 }
$\log q$	9.87850
e	0.99974
Period....	87,000 years.

Parabolic elements are also given, which are almost the same as these.

The same No. contains an article by Prof. Wolf throwing some doubt on the absorption of starlight by Comet 1903 IV. He suggests now that this may have been only a photographic effect produced by the action of the comet's light on the film, and not genuine absorption.

Seven months have now passed without a single comet being under observation in northern latitudes. It is necessary to go back a good many years to find a parallel to this. A. C. D. C.

MINOR PLANET NOTES.—Planets NA, NH prove not to be new, but identical with 505 and 200 Dynamene. A new planet, NJ, of magnitude 14.0, was discovered by Wolf at Heidelberg on March 4. It was near the place of the lost planet 310 Margarita, but is not identical with it.

Other planets are announced as follows :—

Planet.	Date.	Discoverer.	Place.	Mag.
NG ..	1903 Oct. 27	Götz, Wolf.	Heidelberg.	13
NK ..	1901 Jan. 17	Wolf.	"	11
NL ..	May 9	"	"	11
NM ..	1902 Oct. 8	Götz.	"	13
NN ..	1904 Mar. 14	Wolf.	"	12.5
NO ..	14	"	"	13.2
NP ..	14	"	"	12.0
NQ ..	14	"	"	13.3
NS ..	21	"	"	12.2

Another planet, not yet lettered, was discovered by Peters at Washington, 1903, Sept. 28.

NP may be identical with 255 Oppavia.

Dr. Berberich gives, in *Ast. Nach.* 3935, an orbit of 1898 DW, and finds that it is probably not identical with NF.

NM is near the place of DW in 1902, but not identical with it.

NK, NL may be a single planet, in which case it would have an interesting orbit.

A. C. D. C.

OBITUARY.—M. HENRY PERROTIN. The cause of astronomical science in France has been deprived of another of its ablest advocates by the lamented death of M. Perrotin, the director of the observatory at Nice. For more than twenty years M. Perrotin has watched over the growth and directed the energies of that institution. It was his good fortune, through the munificence of M. Bischoffsheim, to be able to erect and arrange a well-equipped observatory to his own design, unhampered by legacies from former benefactors or directors. How the work grew under his hand astronomers have long since recognised and appreciated. As each instrument was completed it was immediately devoted to some special purpose. The meridian instrument was employed to determine the difference of longitude between Paris and the observatory, and to complete the chain Paris-Nice-Milan long before the observatory was in working order as a whole. The fifteen-inch equatorial was at work on double stars, planetary markings, comets, &c., before the large instrument of thirty inches aperture, under the mammoth "floating dome," could be devoted to the more rigorous scrutiny of faint and difficult objects.

It is scarcely necessary here to direct attention to the industry that marked the career of the first director of the Nice Observatory, or to the value of the researches produced by the staff under his guidance and encouragement. The work of M. Thollon on the solar spectrum may serve as a specimen in the department of spectroscopy. The discovery of many minor planets shows the care with which the photographic plates were taken and scrutinised. More particularly as the work of M. Perrotin, personally, should be mentioned his discussion of the inequalities in the orbit of Vesta,

a research to which he devoted much time, interrupted as it must frequently have been by the care of the establishment under his charge. As an observer he was indefatigable, and devoted much time to the study of the faint markings on Venus, on Mars, and on Uranus. Aware that he was working at the extreme limit of visibility, and knowing the tendency for self-deception to creep in and impair the value of such delicate observations, he sought opportunities of making similar measures and records with different instruments, and under varied conditions, in order to remove, so far as possible, the evils of bias and partiality from the results of his researches. Excessive and painstaking care marked his efforts to secure rigorous accuracy.

Apart from his astronomical work, properly so called, in the department of physics, he added another determination to those that have been made on the velocity of light, which we recall here mainly to show the varied character of his researches and the energy which he displayed in whatever he undertook. His life was a busy one, and he did not spare himself. The great monument that he has left behind is the magnificent observatory at Mont Gros, and his greatest service to science is perhaps the activity which he inspired in those by whom he was surrounded. At the comparatively early age of fifty-eight he has succumbed, but he leaves behind him a memory that will be long treasured by all those whose fortune it has been to assist him in earning the reputation that the young observatory at Nice has already won.*

A DETERMINATION OF LONGITUDE: POTSDAM—GREENWICH.—It has been already mentioned in these pages that a determination of the difference of longitude between Potsdam and Greenwich was made by Prof. Albrecht and Herr Wanach in the summer of last year, 1903. The observations were made at Greenwich in the Transit Pavilion, the transits of stars being recorded by Repsold registering micrometers mounted on small transit-instruments of the *coudé* form at one end of the axis. The result of this determination has recently been communicated by Prof. Helmert to the Prussian Academy of Science. Especial care was taken with the exchanges of signals, and some interesting telegraphic experiments made in this connection. The line between Potsdam and Greenwich consists normally of 522 kilometres of land-line between Potsdam and Emden on the North German coast, then 425 kilometres of cable to Bacton near Lowestoft, and 235 kilometres of land-line to Greenwich. At Emden and Bacton the land-lines were joined direct to the cable, but since it is believed that an unsymmetrical arrangement such as this, with more wire on one side of the cable than on the other, does not tend to make the time of transmission the same in both directions, the English Post-Office officials kindly arranged that the signals should pass through a loop, London—Bedford—Leicester—Dunstable—London, 334 kilometres long, and thence to Greenwich.

* From *Nature*, 1904, March 17.

The results of the exchanges of signals showed that the time of transmission, assuming the same in both directions, through this circuit of 1091 kilometres of land-line and 425 kilometres of cable was $0^{\circ}141$, but at some periods of each evening the loop was taken out of the circuit, and repeaters, such as are generally used in the telegraph working, were inserted at Emden and Bacton. With this disposition it was found that the time of transmission was $0^{\circ}079$. Moreover, the difference of clocks determined from the signal exchanges with the repeaters was, in the mean of 24 nights, $0^{\circ}012$ greater than the difference determined by direct transmission without repeaters through the whole circuit with loop. These results depend, of course, on the assumption that the time of transmission is the same in both directions, which, however, is not proved, and it is thought that the above-mentioned discordance in the difference of clocks may result from inequality of these times; and as the symmetrical circuit is more likely to give correct results in this particular, although the time of transmission is larger, this series has been used for the longitude.

The results of the longitude determinations on individual nights show a splendid accordance. Thirteen determinations in May, when Prof. Albrecht was at Greenwich and Herr Wanach at Potsdam, show a range of $0^{\circ}093$. Eleven determinations in June and July, when the observers had changed stations, show a range of $0^{\circ}048$, and the result deduced from these for the difference of longitude between the Greenwich Transit-Circle and the Meridian of the Geodetic Institute at Potsdam is $52^m 16^{\circ}051$, with a probable error of $\pm 0^{\circ}003$.

By help of previously determined values of other arcs, Prof. Albrecht then deduces from this result the difference of longitude between Greenwich and Paris thus :

Potsdam — Greenwich	$52^m 16^{\circ}051$	
Berlin — Potsdam	$1 \quad 18^{\circ}721$	(A)
$\therefore$ Berlin — Greenwich	$53 \quad 34^{\circ}772$	
Berlin — Paris	$44 \quad 13^{\circ}860$	(B)
$\therefore$ Paris — Greenwich	$9 \quad 20^{\circ}912$	

The arc (A) was determined in 1891 by the Prussian Geodetic Institute; the value of the arc (B) used is also the result of a determination made by officers of the same Institution in 1877, so that this value of the difference of longitude between Paris and Greenwich depends wholly on the work of German observers. It is pointed out that if the value of the arc (B) found by Prof. Bak-huyzen in 1893 from a compensation of longitude arcs were used, the final result would be $0^{\circ}03$ smaller than that given above.

THE Bruce Gold Medal of the Astronomical Society of the Pacific has been awarded to Sir William Huggins for distinguished services to Astronomy.

MESSRS. SAMPSON LOW have kindly sent us a copy of the new edition, the eleventh, of Mr. W. T. Lynn's 'Remarkable Comets.' This handy little book has again been brought up to date.

SOME reader in search of bargains in scientific apparatus may like to know that the contents of the late Dr. Common's observatory are in the market. An announcement will be found in the advertisement pages of this number.

Two lectures will be given at the Royal Institution in the coming season by Mr. H. F. Newall on "The Solar Corona."

THE next Meeting of the Royal Astronomical Society will be on Friday, April 8; of the British Astronomical Association on Wednesday, April 27.

FROM AN OXFORD NOTE-BOOK.

WHAT an important feature in astronomical literature the "bulletins" or "circulars" are becoming! This form of publication has so many advantages that it is surprising it was not developed earlier; but now that it has been discovered, and readily adopted by those observatories least hampered by traditions, it will probably make its way steadily. The prototype is, so far as I am aware, the 'Harvard Circular,' though the name "Circular" has not been followed elsewhere. I suppose that the circulars or bulletins are sent to a limited number of observatories and institutions, but only rarely to individuals, and this is of course a disadvantage for isolated workers. These, however, could be reached in various ways, of which the following is only one example:—If brief abstracts of the contents of the circulars (or even full titles of them) were given in the chief journals, such isolated workers would be kept informed of anything likely to be of value to them, and could then write for it to headquarters. To put the matter on a business footing, a small charge could be made. It would of course facilitate the business if some central institution (the Smithsonian?) could undertake this piecemeal distribution of circulars, being supplied from the individual observatories with a number of copies of each circular. But we are not very skilful in devising new methods on this side of the Atlantic, and probably a much better plan will be found by those who have already started the new movement. I only wish to express appreciation of the advantages which the system renders possible, viz.:—(a) rapid publication without waiting for other papers to form a volume; and (b) (though this is a little more doubtful) the facility for re-arrangement under subjects. These might well be extended to more serious memoirs. The inconvenience of delaying one memoir until several others are ready to form a volume has long been felt, and it could be remedied by considering each memoir as a unit to be issued separately. No doubt some people will grumble, notably those who do not want to read the memoirs,

but merely to put a neat volume onto their shelves; but the interests of workers should surely come first?

IN one of the recent circulars, it was pleasant to find a small joke. I don't quite know whether Mr. Lowell will like my calling attention to it, for it is "only a little one," and he so obviously could not help it! But since no system for sending his circulars to everyone has yet been devised, perhaps he will allow me to reproduce here two sentences from his most interesting account (Lowell Bulletin, no. 8) of what he calls the *secular* disappearance of some of the Martian canals. "Neither to deposition of hoarfrost," he says, "such as frequently whitens whole regions of Mars, nor to other circumstance can be attributed its [the canal's] disappearance. Without rime or reason it has simply ceased to be and then as simply come back again." This is at the beginning of the bulletin, which I accordingly read through to see if there were any more; but I became so much interested in his argument that I must have missed them. His conclusion is that the wily Martians have alternative routes; they turn the water from one canal into another so as to irrigate different districts. Whatever we may think of the probability of his views about Mars, there is no doubt of their value as a consistent working hypothesis; the facts range themselves in order splendidly.

VERY little information of an exact kind has been published with regard to the relative speed of logarithms and arithmometers. A small experiment made the other day gave what was, to me, so striking a result that I mention it, chiefly with the view of eliciting more elaborate comparisons if they have been made. It was required to form

$$\xi = Ax + By + Cz \quad \text{and} \quad \eta = Dx + Ey + Fz$$

for 30 values of (x, y, z) : A, B, C, and D, E, F being constants, and all the numbers consisting of five decimal places. The work was done independently by a good computer using logarithms, and by another who had had some weeks' practice with a Brunsviga arithmometer. The former took over four hours for the work, the latter 72 minutes. Three discrepancies appeared on comparing the final results, two due to the machine and one to the computer using logarithms. It may come as a surprise to some that a machine should make mistakes, but it seems to be rather difficult to get a perfectly faultless Brunsviga. After a little wear these machines sometimes make an error, especially if worked very quickly. It was for this reason that I had the work checked by logarithms, but it would appear as though the check were too expensive, and the machine figures must be checked by some independent use of the machine itself. It should be remarked that this particular form of computation is not specially "adapted to logarithmic computation," but I have shown elsewhere that the gain in such adaptations is only slight, and is probably sufficiently compensated by the fact that the 90 logarithms for x , y , and z served for forming both ξ and η . If the speed ratio of more than

3 to 1 is confirmed by the experience of others, it would seem that we should gain a good deal by revising several of our computation-forms and using machines.

A STORY was told me about a dear old lady of strong religious convictions whose curiosity was aroused by the sight of an arithmometer. The details of its working were carefully explained to her, and produced an obvious impression. It was clearly a new idea to her that machinery could be so effective, and at last she gave utterance to the exclamation: "Then there *may* be something in a Chinese praying-wheel after all!"

THE 'Decennial Publications of the University of Chicago,' if we may judge by one or two samples which have already come to hand, will be a truly magnificent series of volumes. There will be ten in all, dealing with all departments of knowledge, from Semitic Languages to Bacteriology; but we are specially interested in volume viii., "Astronomy and Astrophysics," two extracts from which have already reached this side of the Atlantic. The first is "The Spectra of Stars of Secchi's Fourth Type," to which the names of G. E. Hale, F. Ellerman, and J. A. Parkhurst are attached, a contribution to spectroscopy of the first order of importance, which could scarcely have been attempted without a very large telescope. [One of the conclusions reached is that many lines widened in sun-spots are represented by strong dark lines in the spectra of these stars, which suggests that spots similar to those on the Sun may be numerous on fourth-type stars.] The second extract is by G. W. Ritchey, on Astronomical Photography with the 40-inch refractor and with a 2-foot reflector, and contains some beautiful plates (as, indeed, does the other part)—clusters, Moon, nebulae, &c. I hope the number of beautiful presents we get from across the Atlantic will not gradually make us look for them too much as a matter of course.

THE friends of the late Dr. Common will be glad to see Mr. Ritchey's outspoken testimony to the value of the double-slide plate-carrier.

The double-slide plate-carrier, a device originally suggested by Dr. Common and described by him in 'Monthly Notices,' vol. xlix. p. 297, permits very perfect guiding or following to be done, without the necessity of an auxiliary or guiding telescope

. . . I have described the double-slide plate-carrier and its use somewhat in detail, because of the very great importance of this apparatus in long-exposure photography, especially with large telescopes. It has been asserted by prominent astronomers that such difficult objects as the dense star-clusters and the planets could never be satisfactorily photographed, because the very powerful telescopes which are necessary to show these objects satisfactorily are so large and heavy that they cannot be moved with the delicacy and quickness necessary to compensate for the constant irregular tremors always visible with such great telescopes. Experience with the forty-inch refractor, with its enormous weight, the largest instrument thus far successfully used in direct photography, shows that the difficulty is completely solved by the use of the double-slide plate-carrier, in which the mass to be used in making the necessary corrections is two or three pounds instead of ten or twenty tons. It is safe to

assert that for the largest telescopes which could now be constructed, refractors or reflectors, the problem of efficient guiding during long exposures in direct photography is satisfactorily solved.

FROM a South African correspondent :—

I have never come across any mention in an astronomical periodical of Carlyle's few remarks on astronomers, or of the fact that he was once a candidate for the Directorship of the Edinburgh Observatory. It seems that this position was in the gift of Jeffrey of *Edinburgh Review* fame, and Jeffrey and Carlyle were intimate friends. Carlyle asked for the position, and was gruffly refused it by nearly the return post. Jeffrey appointed his secretary, whom Carlyle calls "his taciturn friend with the bleary eyes." Who this was the writer knows not. Carlyle is good enough to say that Jeffrey's nominee did "well enough." Carlyle met Airy once—"a hardy little figure, of edacious energetic physiognomy, eyes hard, strong, not fine." He met Legendre, whose Geometery he had translated in Edinburgh. He was also touched by the hem of Laplace's garment, thus:—"At a meeting of the *Institut* I saw and well remember the figure of Trismegistus Laplace; the skirt of his long blue-silk dressing gown (such his costume, unique in the place, his age and fame being also unique) even touched me as he passed on the session's rising. He was tall, thin, clean, serene, his face, perfectly smooth as a healthy man of fifty's, bespoke intelligence keen and ardent, rather than deep or great. In the eyes was a dreamy smile, with something of pathos in it, and perhaps something of contempt."

It is in every way gratifying to us in this country to know that the Lunar Theory is being worked out by Prof. E. W. Brown, who though at present across the Atlantic is an Englishman, and that Mr. Cowell is worrying the accumulated observations to get at the constants. We have so often neglected our duty in this respect; witness the following words written in 1822 by F. Baily in the Preface to his 'Astronomical Tables':—

Is it not mortifying to reflect that, since the time of HALLEY (the contemporary of NEWTON) this country has not produced a single astronomical table; that although the Royal Observatory at Greenwich has been established nearly one hundred and fifty years, its observations, since the time of FLAMSTEED, have been of no essential advantage to the world, until they have passed through foreign hands, and been returned to this country in the shape of tables formed for various purposes by the successive labours of MAYER, DELANBRE, LALANDE, BÖRH, BURCKHARDT, and BESSEL? Look at the various columns which compose the monthly pages of the Nautical Almanac: there is not a single article of English origin; they are *all* deduced from tables which have been formed by some of those authors to whom I have just alluded. Every table in the present work (I am ashamed to confess) must lay claim to the same origin.

FROM a Greenwich correspondent :—

SCRAPS FROM 'ZADKIEL'S ALMANAC,' 1904.

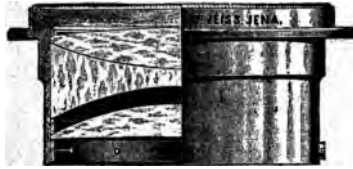
In May we read that "eruptive fever stalks through Russia and Prussia, and may break out in Dublin." Again, in August "the 3rd is troublesome to lovers," and later "the ruler of Saxony is plagued by Saturn." In October "Mars in Virgo excites the Parisians chiefly about the 20th and 28th days, when numerous fires are threatened." And "the 23rd is good for clergymen, barristers, physicians, woollen drapers, grocers, and teachers." How discriminating! In the "general" predictions we read that "Mercury in the fourth house is ominous of several solicitors and clerks incurring punishment." The locality seems here to be indeterminate. Also Uranus "on the verge of Capricornis" necessitates "a watchful eye on the border tribes and the state of Afghanistan," and further, "criminals will give trouble." Lastly, "the weather forecasts are experimental." It is kind of the Editor to warn us not to expect too much.

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EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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DR. GILL, now Astronomer Royal at the Cape, wrote:—"Of the 50
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THE OBSERVATORY,

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No. 344.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 April 8.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker. Seventy presents have been received since the last Meeting, including, amongst others, 'Annales de l'Observatoire de Meudon: Atlas de photographies solaires' (presented by M. Janssen); 'Observatoire d'Alger: Catalogue photographique du Ciel,' 3 fascicules, and Introduction by Ch. Trépied (presented by the Observatory); 'The Rumford Spectroheliograph of the Yerkes Observatory,' and 'Spectra of Stars of Secchi's Fourth Type' (presented by Prof. Hale); 'Radial Velocities of Stars of the Orion Type,' by E. B. Frost and W. S. Adams (presented by Prof. Frost); S. W. Burnham, 'Measures of Double Stars' (presented by the author); Tokyo Observatory 'Annales,' tomes 2, 3, with Report of Solar Eclipse of 1901 (presented by the Observatory).

The President. It may be convenient if I just mention which of the papers we shall take for reading this evening. There is a note by Mr. H. C. Plummer on the distortion of the objective of a micrometer, which has aroused a little attention at one or two observatories, and I think we might take that first. Mr. Bellamy has a note on the number of stars observed at Oxford, and I should like to say a word on the Rousdon Variable-Star Observations. We were expecting to have a paper from Mr. J. Franklin Adams, who has just come from the Cape, but I think I am right in understanding he desires to defer it till next month. Sir David Gill is here, prepared to tell us something about the

Cape Observatory, and, more or less at his own suggestion, I propose to ask him towards the end of the meeting to address us, so that he may know how much time is at his disposal. I know he has sufficient material to occupy the whole meeting. I will first ask Mr. Dyson to say a few words on the question of optical distortion, and perhaps Mr. Russell will follow.

Mr. Dyson then read part of a paper by *Mr. H. C. Plummer* on optical distortion of the object-glass of a micrometer used at the Oxford University Observatory for measuring Astrographic Plates. The distortion had been detected by the difference in the correction for runs found at different parts of the field, and its amount seemed considerable. He added, the same matter has come up at Greenwich in measuring photographs of Eros. There is in use a new micrometer, made two years ago, with an object-glass of just over 3 inches focal length. It is used with double magnification, so that the object-glass is $4\frac{1}{2}$ inches from the object to be measured, and its distance from the focal plane is 9 inches. The field extends $2\frac{1}{2}$ degrees from the centre, and the amount of distortion was found to be a great deal smaller than what Mr. Plummer found at Oxford, and, such as it is, it is very largely eliminated by measuring from the two réseau lines.

The President. When my attention was drawn to this matter, I communicated with Cambridge Observatory, asking if they noticed anything of the kind, and I also wrote to Greenwich. Mr. Russell will be able to tell us as regards the Cambridge instrument.

Mr. H. N. Russell. The Cambridge measuring-machine is of the same general type as those used at Oxford and Greenwich. It appeared at first that there was a considerable distortion of the field, but of a different character from that which had been just described.

If the other machines measure correctly in the centre of the field they would measure too long on both the right- and left-hand sides of the field; but the Cambridge machine measures too long on one side and too short on the other. This can hardly be due to the objective, which must produce a *symmetrical* distortion, but it can be caused by a tilt of the glass scale, which ought to be parallel to the plane of the plate which is being measured.

On removing the eyepiece and examining the reflections from the upper surface of the scale, it was found that the scale was actually tilted about one degree. The magnitude and direction of this tilt agreed very well with that theoretically necessary to produce the observed distortion.

This sort of distortion can be eliminated by taking the means of readings for settings at equal distances to the right and left of the centre. The remaining distortion, which alone can be attributed to the objective, is practically insensible, and certainly would not affect the measured co-ordinates of any star by $\frac{1}{100}$ of a second of arc. The objective appears to be free from optical distortion throughout a field nearly 5° in diameter.

The error due to tilt of the scale will be completely eliminated in the ordinary course of measurement. All plates are first measured in one position, and then turned upside down and measured again. The effect of this error would be to increase the measured co-ordinates of any star in one case, and in the other to decrease them by the same amount, so that the mean is free from error. I think we may conclude that the instrumental distortion in the Cambridge machine, so far as it affects the final result, is practically insensible, being undoubtedly too small to be determined with certainty.

Sir David Gill asked whether the reversal of the plate did not give the same thing.

Mr. Russell. We measure the position of the stars with respect to both the neighbouring réseau lines. The effect of tilt of the scale is always to set the star too far to the right, as seen in the eyepiece. But on turning the plate upside down, the two réseau lines are interchanged, and the effect on the star's measured co-ordinates is reversed in sign.

Mr. Hinks. In one respect the Cambridge machine differs from those used at Greenwich and Oxford. The latter have objectives of the microscope type, the former an objective of the type of the photographic doublet corrected to give a flat field. I consider this difference is of importance, and am confirmed in that view by the fact that the figures just given show that the field of the Cambridge machine is much flatter than that of the Greenwich and Oxford machines.

The Astronomer Royal. I do not think I have anything to add about the objective of the microscope of the micrometer used at Greenwich. Mr. Dyson has mentioned that a diaphragm is used with it, which gives it somewhat the same character as a doublet, different parts of the objective being used for different parts of the field, but the objective itself is, I understand, of the same form as one supplied to Sir David Gill for use at the Cape. From the results it seems to be a very good objective, and to answer the purpose admirably. I think that a very important point in connection with a measuring-machine is to have as great a focal length as possible in the objective, because in that way the distortion of the image is reduced.

The President. We do not seem to be as yet at the end of the small discordances which are occurring in the measurement of photographic plates. Distortion of the microscope-objective might have been foreseen; but I do not know whether it has occurred to anyone to enquire into the source of error that Mr. Russell has spoken of, or to realize that we must "square on" the micrometer reticule or divided scale almost with the same accuracy as we "square on" a telescope object-glass. I will now ask Mr. Bellamy to tell us something about the relative density of stars in different parts of the zone photographed at the Oxford University.

Mr. Bellamy. Having completed the measurement of the 1180

plates for the portion of the Astrographic Survey, zones $+25^{\circ}$ to $+31^{\circ}$, allotted to Oxford, I should like to give a brief summary of the total number of measures. Four years ago I gave some information to the Society about the distribution of stars as derived from the measures of 513 plates. The number of stars measured was 141,539, with an average of 276 stars on each plate, 58.7 stars a square degree, and a ratio of 4.0 to the number of stars in B.D. (Screen.) The number now dealt with is 399,805, with 72.3 stars a square degree, and 339 stars per plate of 4.7 square degrees. (Screen.) On 53 plates in the Milky Way, with from 1000 to 3000 stars photographed in 3 minutes, only those stars showing all three exposures were measured, so that if these extra stars be added the results become 441,929, 375 stars per plate, 79.5 stars per square degree, and a ratio to B.D. of 4.66. (Screen.) It may be mentioned that there are 85,673 B.D. stars in the same area of 55.46 square degrees. The southern zones were photographed about 10 years ago, and show that the plates were less sensitive than those used during the last 3 years for zones $+30^{\circ}$ and $+31^{\circ}$. The plate with the greatest number of stars showing the 6-minutes and 3-minutes exposures is plate 1582, R.A. $19^{\text{h}} 39^{\text{m}}$, $+30^{\circ}$, with 4920 stars. These totals of course do not represent different stars in the sky, but, approximately, $3\frac{1}{2}$ times the number, owing to the overlapping of plates. I have calculated the number of stars per square degree for each hour of R.A., and they agree fairly with the curve given in the previous paper referred to; the chief difference is in the higher level which the revised curve takes almost throughout. (Screen.) One fact becomes very evident in discussing these results in regard to the distribution of stars, and that is, the uncertainty of atmospheric conditions, and the chemical variability in the photographic plate and subsequent processes which they are subjected to, have far greater influence on the number of stars which may be photographed in a given time, say 3 minutes, than a considerable variation in the time of exposure—more than 50 per cent., I believe. I have tried to obtain similar numbers on other plates in those regions where more than about 2000 stars in 3-minutes exposure were shown, but I have not yet succeeded in obtaining anything like such numbers.

Sir David Gill. There is another rather important point which has been brought out by the account which has been given. Whereas the Bonn Durchmusterung is supposed to contain all the stars down to the tenth magnitude, the ratio of the number of stars on a photographic plate to the number in the Durchmusterung shows an increasing tendency in the region of the Milky Way, and it was supposed at first that that was due to the blueness of light of the stars of the Milky Way. Something recently published seems to prove that it was due to nothing of the kind, and that the whole thing was entirely due to the fact that when a man has a great number of stars to observe his ideas about magnitude change, and when there were only a few stars passing through

the field his mental scale of magnitude was different to what it was when there were a greater number of stars.

Mr. Hollis. With regard to what Mr. Bellamy has said, that there are parts of the sky which are particularly dense, and that this density begins abruptly, or, as Mr. Bellamy put it, one comes suddenly on a "wall" of stars, I should like to say that I have noticed the same thing, and that these dense parts are not usually quite in the Milky Way, but generally just at its limits. The densest part of the sky yet counted in the Greenwich Zone happens to be in a region which in most maps is marked as a fork in the Milky Way. Then, again, there is a very dense region in the most northern part of the Milky Way, not within the limits of the galaxy, but just outside it.

The President. I think the meeting will accord a vote of thanks to Mr. Bellamy for his paper; I know that it represents a very great deal of work on his part.

The President. There is one paper which I forgot to mention in my preliminary remarks. Mr. Lewis has presented a paper of considerable importance on double stars—a very extensively compiled catalogue of double stars, very much more than a catalogue—and it is to be printed in the *Memoirs of the Society*, where the Fellows will have an opportunity of studying it in detail; but, no doubt, it would interest the Fellows to hear from Mr. Lewis himself what the memoir is.

Mr. T. Lewis. I am sorry I did not come prepared for any statement, and the papers, which were at the Society, are now in the hands of the printer. When double-star work was in its infancy, Struve, the Russian astronomer, set to work to make a survey of the heavens—very much the same as you have been hearing of to-night—to find out and catalogue all the double stars. Of course, in so doing, he naturally catalogued a number of stars that were found previously by Herschel and South which had not been put in any sort of order, and consequently it so happens that the first large catalogue of double stars emanated from Struve. It took him a number of years, from about 1825 to 1835, to complete the work; but in the meantime he published a preliminary catalogue, and in that preliminary catalogue he gave the approximate places of the stars and a few measures so that other people could get to work on them—he gave stars to the number of 3112. When the large catalogue was completed and the measures arranged in proper order, some stars in the original catalogue were discarded—some hundreds of them—and so his numbers were reduced to about 2640. You will notice that these Struve stars to the number of 533 are marked "R"—that is to say, they were rejected from his catalogue. Those, unless they have been observed once or twice by recent observers, I also reject, because they consist for the most part of stars over 20" apart and are of little interest. The number of stars in the catalogue now before the Society will amount to about 2800. The measures for these have been going on for 60 or 70

years, and I have collected them all—I won't say all, but the majority of them—and discussed them. Of course, individual pairs have been discussed at different times, but the catalogue as a whole has never been attempted. That is the principal part of the work. But there is another feature in the work, and that is, wherever possible the transit-circle observations have been brought into use. The micrometer measures give the relative motion of one star round the other; but if you can by any means get the absolute motion of the principal star, you can determine the masses of the two. This may, in some cases, be obtained from the transit-circle observations. In other words, the orbit is derived from the irregularity of the proper motion. There is one peculiarity I might be allowed to point out, namely, that in most cases the small star by magnitude is in mass equal to or even greater than the other. (Blackboard illustrations.)

Father Cortie. With regard to the point raised by Mr. Lewis, that the magnitudes of some parts of stars are inversely as their masses, I understand that Mr. Lewis's observations of magnitude were visual estimates. It perhaps might be suggested that the visual magnitudes of stars are inadequate expressions of their radiation, and that in comparisons of relative brilliance and mass, account ought also to be taken, where possible, of the spectra of the stars so compared.

Sir David Gill. I am not surprised to hear that in several cases the mass of the apparently small star is comparable with that of the brighter. I have met with the same thing in the case of α_1 and α_2 Centauri. The magnitudes of these stars are roughly in the ratio 1 : 2, but their masses are as nearly as possible the same.

The Astronomer Royal now took the Chair.

The President. I mentioned at a recent Meeting that the Rousdon Variable-Star Observations had been put into my hands to edit and publish. The printing is now completed, and they will shortly be issued. Some of the results attained at the end are of such interest that I think another word about them may be permitted. It may be remembered that I drew attention to the possibility of determining the period by a rather novel method, and I will now show on the screen how well these determinations of period agree with Mr. Chandler's formulæ. Just before they were published, Mr. Chandler gave a revision of his formulæ in the *Astronomical Journal*, No. 553, and it is interesting to note that his corrections in every case but one improve the accordance with Rousdon. Now comes the next point. These periods are not constant in all cases—in fact, in comparatively a small number of cases the period is subject to no change. Mr. Chandler has given for some time back periodic terms in the periods, but he has not suggested any physical origin for these terms, and I am not prepared to give one yet, although it seems to me that there is probably more than one wave of light-variation. I have not gone very far in the discussion because the amount of material was scarcely

sufficient to enable me to proceed far, but some curious results have been obtained. If the light-curves be submitted to harmonic analysis, the different harmonics seem to fall into a regular series. If we group them into 5 groups, the coefficients seem to be all related to one another, at any rate for the first three harmonics. If this provisional result turns out to be well founded, we may be able to separate out the main waves from those which cause the periodic disturbance of this wave. But at present the material is scanty. In attempting to supplement it from other light-curves, I was surprised to find how few had been published, I feel sure there must be material in the hands of observers for forming many well-determined mean light-curves, but, so far as I could discover, very little of it has been utilized in this way. I should be glad to hear of any determinations of *complete* light-curves—not merely the portions near maximum or minimum—if those better informed would kindly help me.

One more interesting point may be noticed. I find that if a curve of solar activity be laid down from Wolf's sun-spot numbers, and the curve submitted to harmonic analysis, the deduced coefficients fit in very well with the formulæ deduced from the Rousdon variables, if we regard a sun-spot maximum as corresponding to a variable star maximum. They will not fit in at all if we regard a sun-spot maximum as corresponding to a variable star minimum.

Father Cortie. The result of Prof. Turner's analysis as to the connection of the maxima of the sun-spot curve and the greatest brilliancy of the variable stars would be confirmed spectroscopically, if it is true, as there is good reason to think, that the bright centres of the H and K lines of the integrated light of the sun are brighter at sun-spot maxima than at the minima. Also the rapid rise of the sun-spot curve to maximum, and its relatively leisurely descent to minimum, is reproduced in the light-curve of the variable stars.

Mr. H. N. Russell. A great many variable stars show bright lines in their spectra at time of maximum which is again a similarity between the Sun at maximum activity and a variable star at maximum brightness.

The Astronomer Royal. I am sure the Fellows present will agree with me that this is a most interesting discussion which Prof. Turner has raised on the variable star observations made at the Rousdon Observatory. It was most interesting to see how he has brought them into accordance, by legitimate methods, with Mr. Chandler's particular work, and I am charmed at the way in which Prof. Turner has worked those observations in with the sun-spot period. It is interesting to see how he has worked from somewhat slender data to show that the maximum sun-spot phase corresponds with the maximum of variability.

The President (resuming the Chair) called upon Sir David Gill for an account of the work at the Cape Observatory.

Sir David Gill. I do not intend this evening to try to give the

Society a full account of the work at the Cape Observatory, for I would rather speak now of only the newer things and spread the subject over another evening. As a matter of fact, I am not quite prepared to describe all the things that I should like to tell you of, nor do I think there is time to deal with them all efficiently in the half-hour that is at our disposal. However, I propose to speak this evening about the new spectroscope which was placed at our disposal by the liberality of Mr. McClean. [Photographs of the McClean spectroscope, in various positions, and spectra of stars obtained with it, were then thrown upon the screen and described by Sir David in detail.]

The President then invited remarks upon the interesting description of the instrument and the work done by it given by Sir David Gill.

Father Cortie. Sir David Gill is to be most heartily congratulated on the success he has attained in reducing the variations of temperature in the prism-box of his spectroscope to a minimum during long exposures. M. Henri Deslandes was, if I remember rightly, the first to point out the error that was introduced into the measurements of the radial velocities of stars by variations in the temperature of the spectroscope during the observations. Since that time several designs of instruments for maintaining constancy of temperature have been invented and attached to spectrographs. Sir David Gill's is exceedingly efficient. The spectra, too, of the stars obtained, and exhibited on the screen, are remarkably beautiful.

Major Hills. There is one subject to which I desire to call attention, though I do not wish to enter into any argument upon it. Sir David has referred to the appearance of the iron lines as being a proof of higher temperature, and I should like to ask Sir David if he is quite convinced that the appearance is due to higher temperature.

Sir David Gill. No, I am not.

Mr. Newbegin. Can Sir David give the Society an idea of the amount of motion indicated by the displacement of the lines that he has shown?

Sir David Gill. About 105 kilometres per second displacement divided between the Earth and the star. At that time the Earth was moving away from the star at the rate of about 24 kilometres a second. We usually talk of the velocity of a star's motion with reference to the Sun, but what we have been speaking of is the velocity of the star with reference to the Earth, and that amounted to 105 kilometres per second. I should have said that these photographs were taken, some by Mr. Lunt and some by Mr. Goatcher, both of whom have shown much skill in this class of work at the Cape.

The President. I am sure we shall all return our thanks to Sir David Gill for what he has told us this evening, and also accord

him a hearty welcome back to England, and trust that he will return in the best of health. I have reason for thinking that his health has not been good lately, and I hope that his visit home will do him much good. He will be amongst us during the summer, and I trust that we shall have the opportunity of hearing something more of other instruments that he has under his charge.

The following papers were announced and partly read:—

John Tebbutt. "Observations of the Minor Planet (324) Bamberga at Windsor, N. S. Wales."

Thomas Lewis. "Measures of Double Stars in Struve's 'Mensuræ Micrometricæ,' collected and discussed."

A. M. W. Downing. "Corrected Continuation of Brünnow's 'Tafeln der Flora.'"

Asaph Hall. "Note on Elliptic Motion."

H. H. Turner. "The Rousdon Variable-Star Observations."

P. H. Cowell. "Some Further Analyses of the Moon's Errors of Longitude, 1847 to 1901."

H. C. Plummer. "Note on the Optical Distortion of the Microscope of the Oxford Machine for Measuring Astronomical Photographs."

F. A. Bellamy. "An Analysis of the Results of the Measurement of the 1180 Plates in Zones $+25^{\circ}$ to $+31^{\circ}$ allotted to the University Observatory, Oxford, in connection with the International Astrographic Chart."

The following gentlemen were elected Fellows of the Society:—

John Borthwick Dale, M.A., Assistant Professor of Mathematics, King's College, London; "Myosotis," New Malden, Surrey.

Rev. William Charles Eppstein, Reading School, Berks.

William Edward Rolston, Solar Physics Observatory, South Kensington, S.W.

Walter Nuttall, B.A., Halford, Whitworth, Rochdale.

The following Candidates were proposed for election as Fellows of the Society:—

Edward Barlow, Harewood, Andover, Hants (proposed by Major P. MacMahon).

Major C. Leigh-Lye, 15 Mayfield Avenue, Chiswick, W. (proposed by A. C. D. Crommelin).

The following were proposed by the Council as Associates of the Society:—

Henri Deslandres, D. ès S., F.R.A.S., Observatoire d'Astronomie Physique, Meudon, S.-et-O., France.

C. D. Perrine, Lick Observatory, Mount Hamilton, California, U.S.A.

George W. Ritchey, Yerkes Observatory, Williams Bay, Wisconsin, U.S.A.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1904 March 30.

President: S. A. SAUNDER, M.A., F.R.A.S., in the Chair.

Secretaries: A. C. D. CROMMELIN, B.A., F.R.A.S.
J. G. PETRIE, F.R.A.S.

Mr. J. G. Petrie read the Minutes of the previous Meeting, which were confirmed.

Mr. A. C. D. Crommelin read the list of presents received, and the thanks of the Meeting were accorded to the respective donors.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of three new members was unanimously confirmed.

Mr. W. T. Lynn read a paper entitled "Prof. Stockwell and the Nebular Hypothesis." The writer observed that the nebular theory, as generally understood, was due to two astronomers, Laplace and Herschel, the latter founding his views on his observations of nebulae, considering them as systems in different stages of formation. Laplace did not treat his hypothesis on the whole mathematically, and Prof. Stockwell had shown, in a recent number of the *Astronomical Journal*, that his assumptions were incorrect. Prof. Stockwell stated, as the result of his investigations, that: "If two planets are moving in the same plane and at the same mean distance from the Sun, and are situated at an angular distance of less than 60° apart as seen from the Sun, their mutual perturbations will cause them to recede from each other until their distance apart becomes equal to 60° ; and they will always remain in a condition of stable equilibrium at that distance apart, and will revolve round the Sun for ever free from mutual disturbance." If, however, these bodies were placed at a distance of 180° apart they would be free from mutual disturbance, but the position would be one of unstable equilibrium; a slight derangement from any cause would produce cumulative effects by their attraction, which would go on until they had reached the angular distance of 60° , the position of stable equilibrium. A homogeneous ring of satellites would, however, be in a condition of stable equilibrium, as we saw in the case of Saturn's rings. Prof. Stockwell reserved for future consideration a more general discussion of the nebular hypothesis, but hinted that there were other difficulties connected

with the leaving behind of a ring of nebulous matter, which, in fact, involved conditions in the rotation of the primitive nebula, and its variation of velocity, for which the founder of the theory neither provided nor even suggested any probable cause.

Mr. Seabroke asked whether the same thing would happen if the planets were situated at less than 60° apart.

Mr. Lynn said he presumed so, because 60° was the only position of stable equilibrium.

Mr. Crommelin was rather surprised to see the result of Prof. Stockwell's investigations. He had always imagined that Laplace had gone into the matter of a ring coalescing into a single body mathematically. It seemed, however, that he had assumed it *a priori*. It was difficult to see how the result of Prof. Stockwell's investigations could be reconciled with the nebular hypothesis, at the same time we did see these nebulae in the heavens form series of rings, one inside the other, and we saw detached masses, apparently, that had come off some of the rings; so that he had always, since he had seen the wonderful photographs of the spiral nebulae, thought that was the way our own system arose.

Mrs. Herkless said that Prof. Darwin regarded the Andromeda nebula as a strong evidence of the truth of the nebular hypothesis.

Mr. H. Keatley Moore asked whether the nebulae were not spirals rather than rings when closely looked at?

The President instanced the nebula in Lyra as a ring.

Mr. E. W. Maunder said that Prof. Holden thought that even the nebula in Lyra was really spiral.

The President said that the whole question was, of course, an extremely difficult one. They could not have in the universe such a simple state of things as the Sun and two planets at an angle of 60° without any other disturbing bodies.

Mr. Maunder then gave the substance of a paper by Mrs. Maunder and himself, on "The Oldest Astronomy, Part III.," illustrated by lantern-slides.

Mr. Maunder remarked that their first paper communicated to the Association, 1898, June 29, dealt with the subject of the place and date of origin of the constellation figures, concluding that they were designed about 2800 B.C. The second paper, communicated 1899, May 31, dealt with the evidence afforded by the constellation figures themselves, that they formed, broadly speaking, a single scheme. The object of the third paper was to consider approximately the date when Taurus ceased to be considered the first sign of the Zodiac, and that place was taken by Aries.

It was clear that Taurus was once considered the premier sign. The monuments of the Mithraic worship gave the first place in importance in its symbolism to the Bull, and many of these monuments were as late as 300 A.D., showing that the tradition of the association of the Bull with the spring equinox still lingered. Similarly, a figure of the Egyptians deity Serapis shows him as the sun-god, bearing on his robe Taurus, Leo, Scorpio, and Aquarius,

the primitive guardians of the four colures. But at present, and for many centuries, Aries had been considered the first sign. Manilius and Ptolemy gave that sign as the leader, as did Hipparchus as early as 150 B.C. The entire scheme of astrology takes Aries as the first sign, and therefore the change of leadership must have occurred before astrology was reduced to a definite system. The change is generally assumed to have taken place when the "equinoctial point" passed from the constellation Taurus into the constellation Aries, that is about 1700 B.C., but the date of the recognition of Aries as the leading sign must have fallen much later.

There were many indications that astronomy began to experience a great development about 700 B.C. or a little later, and that about that time Aries was accepted as the leading sign of the Zodiac instead of Taurus. The presence of *Hamal*, the first bright star of Aries, on the vernal colure, the equinox tablets from the Kouyunjik mound, the references in the Creation poem, the discoveries attributed to Thales, all led to about the same period, and this agreement could hardly be accidental. The great intellectual quickening in Assyria, of which the literary activity of the reign of Assur-bani-pal was a symptom, must have extended to the field of astronomy.

Mr. Lynn thought we were rather apt to forget that different nations had begun the year at different seasons. The Jew had an ecclesiastical year beginning at the spring equinox, but their civil year began in the autumn. We followed the reformed calendar of Julius Cæsar, and began near the winter solstice. The Greeks began about midsummer, and the Turks used nothing but the Moon; the difference between the Hegira and our era was therefore continually changing.

Mr. Crommelin asked whether the astronomy of Egypt went at all hand in hand with that of Chaldea. He believed that the pyramids were built a thousand years or more before the time of which Mr. Maunder had spoken, and they indicated considerable astronomical knowledge.

Mr. Maunder said that he knew very little about Egyptian astronomy, but they evidently managed their year somewhat differently to that of the inhabitants of the Euphrates Valley; the latter undoubtedly used the lunar month, while the Egyptians used, more strictly speaking, the solar year.

Mr. Maunder then gave an account of three recent Memoirs of the Yerkes Observatory, illustrated by a number of slides reproduced from the photographs illustrating those Memoirs.

Mr. Crommelin said Mr. Maunder had brought out in a very striking way the vast interest of the work going on at the Yerkes Observatory.

The President desired to add his testimony to the immense value of the work done by the Yerkes Observatory. When it was remembered that it was one of the youngest of the observatories,

not more than 10 years old, the progress made in almost every branch of astronomy must be admitted to be enormous.

Mr. Crommelin read a letter from *Dr. F. J. Archenhold*, of the Treptow Observatory, in Berlin, relative to the Solar Eclipse of August 1905.

The President said the Council had appointed a Committee to consider the question of making arrangements for observing the eclipse.

Mr. G. F. Chambers gave the result of his enquiries as to the best points in Spain for the observation of the eclipse. As regards duration there was nothing to choose, but as regards climate the north coast was particularly promising and satisfactory in the month of August. On the other hand, along the Mediterranean littoral, the heat at that time of the year was so excessive that all his correspondents pronounced against any attempts being made by English people to face it, for it was tropical heat.

The President thanked *Mr. Chambers* for his interesting statement, and the Meeting adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE monthly Meeting of this Society was held on the evening of Wednesday, April 20, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, President, in the Chair.

A paper by *Mr. W. L. Dallas*, on "The Variation of the Population of India compared with the Variation of Rainfall in the Decennium 1891-1901," was read. The author showed that during the 4 years 1891-95 the rainfall was generally normal or heavy over nearly the whole country, and during the 6 years 1895-1901 the rainfall was greatly deficient. During the former or "wet" period the rainfall was deficient over Upper Burma and Madras, was normal over the remainder of Burma, Assam, Bengal, and the west coast of the Peninsula, and was excessive elsewhere; while during the latter or "dry" period the rainfall was again deficient over Upper Burma, normal or excessive over the remainder of Burma, Assam, Bengal, the United Provinces, the North-West Frontier Province, and the south of Madras, and was deficient elsewhere, most so over Rajputana and neighbouring areas. The general census of India on March 1, 1901, showed the total population to be 293,475,477, which, excluding the territories not included in the 1891 census, was an increase of only 1.3 per cent. The population had thus failed to increase according to the normal rate during the decade. Part of this failure was no doubt due to epidemics. The author, however, shows that there is an unmistakable relationship between the variations of the population and the variations of rainfall during

the dry years. The area within which the most serious decrease of population occurred coincides almost exactly with the area of greatest deficiency of rainfall.

A paper by *Mr. J. B. Cohen*, on "The Cause of Autumn Mists," was also read, in which the author describes some experiments which he made on Coniston Lake some time ago.

EVENING MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

GEODETIC SURVEYS.

ENCOURAGED by the success of the additional Meeting at Burlington House on November 27, 1903, Prof. Turner, as President, invited the Fellows of the Society to attend a similar Meeting on Thursday, April 28, at 8 o'clock in the evening, when a discussion on Geodetic Surveys of the Empire would be opened by Major Hills.

The attendance was fairly large, nearly fifty persons being present, and was thoroughly representative of the subject set down for discussion.

Major Hills, having explained the purposes of a survey to be not only the accurate description of the positions of points on the Earth's surface for the making of maps, but also the measurement of the actual lengths of long arcs for comparison with their angular measurement for finding the figure of the Earth, proceeded to describe different surveys in detail. The Ordnance Survey of the British Isles consists of a network of triangles covering the whole area, and at the beginning this plan was proposed by Col. Lambton for the Survey of India, but was unnecessary and found impracticable for so large a country, and Col. Everest conceived the scheme of making long, accurately-measured chains of triangles called the primary triangulation, which divided the country into five areas, each area being filled in by measurements of less accuracy called the secondary and tertiary triangulations. The long arcs of the primary triangulation are available for purposes of geodesy, the secondary and tertiary for map-making. Major Hills said it had been calculated that at the end of a line of tertiary triangulation 600 miles long the error of a position might be 45 feet. A displacement as this would be imperceptible on ordinary maps, and taking this for basis, he suggested as an axiom in planning a survey that no point of a tertiary line should be more than 150 miles from a primary and of a secondary more than 500 miles. He then showed a map of the survey now proceeding in Africa, and a scheme that he had recently planned for the Survey of Canada, ending with some remarks on the work of the Ordnance Survey, which, though excellent at the time of its initiation, was not of the accuracy now demanded by modern geodesy. The sum of the measured angles of a triangle should necessarily be 180° .

plus the spherical excess. The average discordance from this, called the triangular error, in the work of the Survey is 3", whereas modern work gives an average error of about 1". It was suggested that a primary triangulation should be made.

Sir David Gill spoke on the Geodetic work in Africa. Besides the survey of Cape Colony and Natal, which extends to Kimberley, and the determination of the boundary between German West Africa and British Bechuanaland already accomplished, the meridian line 30° E. of Greenwich has been laid down from the Limpopo to the Zambesi, and the determination of the parts of the same meridian from Natal to the Limpopo, and from the Zambesi to Lake Tanganyika, is now being done. Northward of this Germany would no doubt continue the line, and *Sir David Gill* hoped that the Foreign Office and Lord Cromer would see their way to having the meridian laid down from Cairo southward, so that at no far distant date the 30th meridian would be measured from the Cape to Cairo, and might then be joined to the Russian measurement of the same meridian.

Prof. G. H. Darwin, as representative of England in the International Geodetic Association, spoke of the good results following from the adherence of England to this Association, and the good opinion that Continental geodesists had of the survey work now being carried on in Australia.

Major Close spoke on the work of the Ordnance Survey. Since Col. Clarke's retirement in 1881 there had been a feeling that the work regarded as geodesy was finished. He gave instances where Ordnance Survey marks of some importance had been inadvertently or ignorantly displaced, and supported Major Hills's view that a new triangulation should be made.

Mr. G. F. Chambers asked a question relative to the accuracy of the Survey, and some remarks from *Prof. Turner*, who was in the Chair, closed the discussion.

Adjournment was then made to the Library, where some interesting ancient books and other exhibits were on view.

The Spectrum of Antarian Stars.*

THE distinguishing feature of the spectra of the Antarian Stars is the system of apparently dark flutings, sharp towards the violet and fading off towards the red end of the spectrum. The principal flutings are well seen in Antares, but they are more strongly developed in the spectra of α Herculis and σ Ceti, in which stars additional details are also seen. These flutings have not hitherto received a definite chemical interpretation, and it has been uncertain, owing to the possibly misleading effects of contrast, whether

* Abstract of paper read by Mr. A. Fowler before the Royal Society, 1904 March 3.

the spectrum was to be regarded as one consisting wholly of absorption flutings fading towards the red, or as one partly consisting of emission flutings fading in the opposite direction.

The purpose of the present communication is to state the nature of the evidence which indicates that the spectrum is essentially an absorption spectrum, and that the chief substance concerned in the production of the flutings is titanium, or possibly a compound of that element with oxygen.

The evidence in favour of a titanium origin for most of the flutings depends on such a large number of coincidences that it is almost independent of a very precise knowledge of wave-lengths.

The flutings in question come out in the arc spectrum of titanium oxide, if the precaution be taken to provide a liberal supply of material and to use a very long arc, taking care also that the image of the "flame" is projected on the slit of the spectroscop. They are also seen in the arc spectrum of the chloride under similar conditions. Numerous lines accompany the flutings produced in this manner, and some of the details are consequently masked or not recognized without careful study of the photographs.

So far the flutings have not been very successfully produced in the oxyhydrogen flame; they are visible in the flame spectrum of the fumes from the chloride, but their observation is difficult on account of the bright continuous spectrum.

The best representation of the flutings has been obtained by passing a spark, without jar, though the fumes of oxychloride which rise from the chloride of titanium on exposure to air. Under these circumstances the lines which appear are not numerous, and some of the secondary flutings which are masked by lines in the spectrum of the flame of the arc are readily detected, in spite of the continuous spectrum which is also present. The few lines which do appear in this spectrum are probably low temperature lines which may be found of special importance in the cooler stars.

The paper proceeds to give comparison of the flutings in the spectrum of titanium with those recorded visually in the stars by Vogel and Duner. It is seen that eight of the ten bands recorded by these observers agree within the possible limits of error with the flutings of titanium, and it is to be noted also that the only one of the principal titanium flutings which is not represented in the stellar spectrum is out of range in the extreme red. Comparison of photographs of the spectra of the elements and of stars is also striking. It is seen that the details of the titanium flutings are reproduced with remarkable fidelity in the stellar spectra, and more especially in α Ceti. In the latter spectrum the number of flutings recorded is slightly greater than in the case of titanium, but it is by no means certain that every detail of the titanium spectrum has yet been photographed. It is possible also that some of the features described as flutings in the stellar spectrum may be groups of lines, and in at least one instance (4437) a fluting has been classed as a "wide line."

The points of difference are very slight, and are mostly in the less refrangible part of the spectrum, where the reductions of the stellar spectra present the greatest difficulty.

The general agreement is such as to leave no reasonable doubt that titanium is the main factor in the production of the dark flutings which characterize the Antarian group of stars.

This explanation of the dark flutings suggests that the appearance of bright flutings in the Antarian spectrum arises chiefly from effects of contrast. It does not, of course, exclude the possibility of the presence of bright flutings, such as might be indicated by local brightenings which are not exactly in coincidence with the edges of dark flutings.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Galileo and Marius.

GENTLEMEN,—

In the January 1904 number of the *Observatory* there is a letter on the disputed question of the first observation of Jupiter's satellites—a question which has been raised afresh by Messrs. Oudemans and Bosscha in 'Archives Néerlandaises des Sciences exactes et naturelles, publiées par la Société Hollandaise des Sciences à Harlem.'

Although referred to directly by these gentlemen in their memoir, I do not think it necessary just now to reply to what concerns myself personally; and besides it would be premature in every way to say anything until *all* the writings of Galileo relating to the Medicean Stars have been published in their integrity. This will be done in the course of next year at latest, in the National Edition of Galileo's Works which I am editing, and in the course of which I have to resolve difficulties of such a kind as cannot be adequately understood by those who have not seen and studied the autograph MSS., and who form their opinions on the inaccurate and incomplete publication brought out by Albèri in 1842-56. However, I will now permit myself to remark that Messrs. Oudemans and Bosscha have been led to make certain statements which I think more mature consideration will not permit them to repeat integrally and in a form so absolute.

With these, I repeat, I do not wish to occupy myself at present. Here I desire only to rebut what I find asserted in the letter of Mr. Lynn above referred to, where an argument used by Messrs. Oudemans and Bosscha, but void of any real foundation, is made the basis of a rather sweeping assertion. In their memoir we read (p. 140):—"Galilée se plaignit auprès du Prince Frédéric Cesi et le pria de demander aux membres de l'Académie dei Lyncei

‘de quelle manière il fallut répondre à Simon Marius, usurpateur du système Jovial, s’il fallut le faire en écrivant à Kepler ou au Margrave de Brandenbourg.’ Après ample délibération les Lynceans répondirent qu’ils préféraient qu’une lettre fût écrite à Kepler, comme étant un astronome de la même Allemagne et bien informé, l’autre manière offrant quelque difficulté. Il paraît que Galilée, se ravisant, a estimé que la première manière présentait également quelque difficulté ; à moins qu’il ne faille admettre que la réponse de Kepler n’ait été nullement satisfaisante. Il est certain qu’une réponse de Kepler n’a jamais été publiée, ce que Galilée n’eût pas manqué de faire si Kepler avait jugé que les torts étaient du côté de Marius.”

Leaving aside the inferences which are quite arbitrary and unsupported by any documentary evidence, it is clear that in this passage Messrs. Oudemans and Bosscha do not affirm, as they could not affirm, that Galileo did in fact refer the matter to Kepler; yet Mr. Lynn says positively he did do so. His words are (p. 63, note):—“Prof. Oudemans points out that reference was made by Galileo to Kepler, but that the answer seems to have been suppressed.” Where is the proof that a reference *was* made to Kepler? and, above all, where is the ground for the insinuation that his reply was suppressed (and inferentially) either by Galileo himself or by his editors? Happily, the one and the others, commencing with Vincenzo Viviani and ending with myself, have such a well-founded reputation for literary honesty that they can afford to laugh at such insinuations.

As a matter of fact, I think I can say with absolute certainty that Galileo did *not* refer to Kepler, because he already had good reasons for doubting Kepler’s loyalty (on the occasion of the Martin Horky episode), and because their relations, at first so amicable, suddenly cooled down after the first astronomical discoveries of the great Italian.

In fact, not counting the purely official letter of Galileo to Kepler of August 28, 1627, the relations of the two men may be said to have ceased towards the end of 1611, Galileo’s last letter to Kepler which has come down to us being dated August 19, 1610, and Kepler’s last to Galileo May 28, 1611. These altered relations are further indicated by Kepler’s letters to third parties, in which his references to Galileo show quite other than a benevolent disposition towards his fortunate competitor for fame.

Yours faithfully,

Padua, 1904, Mar. 6.

ANTONIO FAVARO.

[Prof. Favaro’s reputation and self-denying labours are so well known to us all that I am sure, on further reflection, he will see that the question of his suppressing anything in this matter could not come in. Surely if anyone was anxious to prove a point to the satisfaction of another, and left it to that person to decide which of two others he should refer to as umpire, it is fair inference that he

did so refer it. That Galileo suggested Kepler as one of the proposed alternative referees shows that he did not then consider himself debarred from writing to him. Possibly Kepler preferred not to reply to an accusation that Marius had claimed the first discovery of the satellites of Jupiter, which he certainly did not, since he gave his dates by the Julian reckoning. But Galileo was evidently determined to allow no one the credit of an independent discovery of celestial bodies, which he considered an honour reserved for himself alone. Prof. Oudemans has clearly proved that Marius had for some time been in possession of a telescope quite equal to showing the satellites; and that being so, why should we doubt that one who is known in many ways to have been a careful and accurate observer did actually see them? We all feel, with Prof. Favaro, that the honour of departed astronomers is dear to us; but there is no respect of persons in this, and it applies as much to Marius as to Galileo. The accusation of forgery, be it remembered, was made against the former by the latter.—W. T. L.]

An Eclipsing Star of Long Period.

[Translation.]

GENTLEMEN,—

Under this title Miss Clerke has written, in the March number of your magazine, about my work "Enquiries into the Light-changes in ϵ Aurigæ." As every reader of Miss Clerke's article who has not read my work also must form an altogether wrong idea of its aim, I find myself compelled to make a statement of the result of my enquiries.

Apart from the last ten lines, my work is exclusively a photometrical investigation of the change of brilliancy of ϵ Aurigæ without any theory or hypothesis as to the cause of the light-variability. From the reduction and discussion of more than 2000 observations of brightness of the star which were made between the years 1842-1903, and those all by good observers, I proved the law of the changes of light of ϵ Aurigæ, and the result at which I arrived I gave (some unessential numbers and words being omitted here) on page 114 of my work in the following sentences:—

"The light-change of ϵ Aurigæ has a period of 27·12 years. Usually the star has a magnitude of 3·35; at minimum it declines uniformly in 207 days to the extent of 0·73 magnitude. It remains at this minimum for 313 days and then increases in 207 days to its former brightness, at which it continues for 25·13 years, the whole duration of the light-change lasting 1·99 years. The middle of the last minimum took place on 1902 March 31.

"The numbers given are still somewhat uncertain. It is possible that the minimum of 1874-75 differed from those of 1847-48 and 1901-02, and further it is not disproved, although

improbable, that minor light-fluctuations occur to the extent of 0.3 magnitude.

"From the above it will be gathered that ϵ Aurigæ is of the Algol type."

But the expression "Algol type" does not imply anything as to its constitution. In Müller's 'Photometry of the Stars,' page 471, the definition given of this type corresponds closely with that of ϵ Aurigæ. After the above sentences the last ten lines, giving work which Miss Clerke calls "Dr. Ludendorff's theory," follow:—

"This star is interesting on account of the length of its period, it being by far the longest yet found for a regular variable, as well as for the long duration of its eclipse. The spectrum shows that in ϵ Aurigæ we have two distinct bodies before us. The actual period is thus perhaps $2 \times 27.12 = 54\frac{1}{2}$ years, and it would follow that the even and uneven minima are not exactly alike. It is to be hoped that spectroscopic observation will give a closer explanation of this remarkable system."

This is all that I have said about the constitution of the star. The brevity of the sentence and the mode of expression make it, I think, clear that I attached little weight to the above-mentioned "Hypothesis," especially as there are other objections to it besides those adverted to by Miss Clerke. Indeed I regarded as premature any further discussion of the constitution of ϵ Aurigæ. I am at a loss to understand how Miss Clerke could consider this hypothesis as the main result of my work.

Yours faithfully,

Potsdam, 1904, March.

H. LUDENDORFF.

[It was far from my thoughts to cast any discredit on Dr. Ludendorff's important and authoritative photometric work. My little article in the *Observatory* made, in fact, no pretence at dealing with it directly, but merely touched on an especially interesting episode arising out of it.—AGNES M. CLERKE.]

Stellar Distribution.

GENTLEMEN,—

Mr. Gore's letter on this subject in April issue is suggestive. The theoretic ratio of 4 to 1 (or rather 3.984 to 1) is that between the stars of the n th magnitude and those of the $(n-1)$ th magnitude. The theoretic ratio between the stars of the n th magnitude and the total number of stars brighter than that magnitude is nearly 3 to 1. But, of course, when the number of stars is very small, theoretic considerations are not applicable. The brightest star in the portion of the sky in question is of the third magnitude. The stars of the 4th magnitude ought theoretically to be four times as numerous as the stars of the 3rd magnitude and three times as numerous as the total number of brighter stars. But the stars of the 3rd magnitude and the total number of brighter stars are in this instance the same, and theory pronounces with

equal authority in favour of 3 and 4 for the stars of the fourth magnitude. The actual number is 2.

Prof. Newcomb finds that a ratio of 3.5 to 1 for the stars of each successive magnitude keeps nearer to the results of observation than the theoretic 4 to 1. But Prof. Pickering, basing his results on photometric measurements, arrives at a ratio of about 3.13 to 1. In the region of the Pleiades, however, it is clear from Mr. Gore's figures that neither of these ratios are ever approached. I suspect that some of the faint stars were blotted out by the nebulae which abound in the Pleiades and others perhaps lost in the over-exposure of the brighter stars. But these considerations will not account for the wideness of the departure. Omitting the stars of the 7th magnitude, where there is an obvious anomaly, and taking the square root of the ratio between the 6th and 8th, the results for successive magnitudes are as follows:—

Fourth (compared with third), ratio 2.0; fifth, ratio 1.5; sixth, ratio 1.7; eighth, ratio 1.5; ninth, ratio 1.5; tenth, ratio 1.6; eleventh, ratio 2.0; twelfth, ratio 2.0; thirteenth, ratio 1.4; fourteenth, ratio 1.1; fifteenth, ratio 1.6; sixteenth, ratio 1.7. With the exception of the 7th magnitude, which is decidedly anomalous (but the balance is restored by the eighth), and the fourteenth, in which the expected increase is not realized, the rate of increase varies from 1.4 to 2.0, which is probably as near as could be expected, but the average is less than half the ratio assigned by Newcomb and little if anything more than half that of Pickering. Whether this result is due to the presence of a cluster I leave to be considered.

Assuming the derangement in the figures to be due to a cluster containing a mixture of large and small stars at nearly the same distance from us, we might expect that when we once got clear of the cluster the ratios of Pickering or Newcomb would be approximately realized. But (assuming that there is no difference between them and Mr. Gore with regard to the magnitudes of very faint stars) the conclusion appears to be that even up to the sixteenth magnitude a considerable proportion of the stars in this region belong to the cluster. Assuming the ratio for the stars not belonging to the cluster to be only 3.0 to 1, if there were 6 such stars of the 12th magnitude there would be 18 of the 13th, 54 of the 14th, 162 of the 15th, and 486 of the 16th; and as the total number of stars of the 16th magnitude seems to be only 448, it would follow that (assuming the ratio of 3 to 1) out of 113 stars of the 12th magnitude situated in this region, at least 107 belong to the cluster of the Pleiades. With small numbers anomalies may no doubt occur, but I think (unless the distribution of the stars beyond the cluster is of a very unusual kind) we may safely set down 100 of these stars as belonging to the cluster.

It is hardly necessary to add that neither Prof. Newcomb's nor Prof. Pickering's ratios have been tested by stars of magnitudes nearly as low as some of those mentioned in Mr. Gore's table. I

think it unlikely that the ratio between the stars of the 16th and those of 15th magnitude is, on a general average, as high as either 3.5 to 1 or 3.13 to 1. For this reason the grounds for concluding that much the greater part of the fainter stars comprised in the photograph in question belong to the cluster cannot be regarded as altogether satisfactory. Further research will no doubt throw further light on this subject, but any consequences drawn from the assumption of a law of distribution will necessarily fail when applied to a region which includes a prominent cluster.

Truly yours,

16 Earlsfort Terrace, Dublin.

W. H. S. MONCK.

λ Andromedæ.

GENTLEMEN,—

I would like to call the attention of astronomers engaged in the determination of stellar parallax to the case of the spectroscopic binary *λ Andromedæ*, which seems a remarkable one. The binary character of the star was discovered by Professor Campbell, of the Lick Observatory, in 1899. The period is about 19.2 days, and the orbital velocity 5.6 miles a second, the companion being a dark body. Assuming that the components are equal in mass, and that the orbital plane is in the line of sight, I find from the above data that the mass of the system would be only 0.012 of the Sun's mass. If we assume that the plane of the orbit is inclined 60° to the line of sight, the mass would be increased eight times, and would then be about $\frac{1}{10}$ of the Sun's mass. Considering that the star is of the 4th magnitude, this small mass seems remarkable, and would suggest that it is comparatively near the Earth. To reduce the Sun to a star of the 4th magnitude, it would be necessary to remove it to the distance of a star with a parallax of $0''.158$. Now a body of $\frac{1}{10}$ of the Sun's mass, and with the same surface-luminosity, would be 4.64 times fainter than the Sun. The square root of this is 2.153, so in order that it should shine as a star of the 4th magnitude, its parallax should be increased in this proportion. The parallax, therefore, would be

$$0''.158 \times 2.153 = 0''.34.$$

This would be the parallax of *λ Andromedæ* if its mass is $\frac{1}{10}$ of the Sun's mass, and if it had the same surface-luminosity as the Sun. But as the spectrum of *λ Andromedæ* is K, that of the Sun being G, it is probable that its surface-luminosity is less than that of the Sun. We might therefore infer that its parallax is slightly larger than that found above. It would therefore seem probable that the parallax of *λ Andromedæ* is not less than $0''.34$, and an effort should be made to measure it. It has a considerable proper motion, which, according to the Greenwich 10-year Catalogue, is $0^s.0157$ in R.A. and $0''.425$ in declination.

Yours faithfully,

J. E. GORE.

Dublin, 1904, April 2.

The coming Eclipse and its Past History.

GENTLEMEN,—

By the above expression it is obvious that I am referring to returns of a solar eclipse at the end of successive eighteen-year periods, these being sometimes spoken of as recurrences of the same eclipse. In this sense the eclipse of next year (preparations for the observation of which are now being discussed) can be traced back to the year 1743, when it occurred on the 23rd of May, but partial and visible only in a small part of North-eastern Europe. On the 3rd of June also, in 1761, the eclipse was only small, and its locality was confined to North-eastern Europe and the north of Siberia. No record, so far as I am aware, exists of either of these having been seen, and the like may be said of the partial eclipse which occurred on the 14th of June, 1779. When the eclipse recurred on the 24th of June, 1797, it was total, but the central line was confined to the Arctic Ocean and parts of Siberia. The central line of the total eclipse of the 7th of July, 1815, passed over the North Pole, and that of the 17th of the same month in 1833 was restricted to regions near it. The next recurrence (on the 28th of July, 1851) was more accommodating, and as the time for eclipse expeditions had now commenced, it was carefully observed in Sweden and Norway by several English astronomers, including Airy, Dawes, and Hind, who described the appearance of the so-called rose-coloured protuberances. From our present point of view, however, that eclipse is chiefly remarkable for the first application of the "prentice hand" of photography in eclipse-observation, Berkowski having taken a daguerreotype of it (exposure 1 minute 24 seconds) at Königsberg, a reproduction of which is to be found in vol. xli. of the 'Memoirs of the R. A. S.' (from vol. xxvi. of the Königsberg Observations), as I remarked in vol. xxiii. p. 376 of the *Observatory*.

From 1851 we pass on to 1869, when the total eclipse of the 7th of August bestowed its chief favours upon the American astronomers, who did not fail to take full advantage of the opportunity. The line of centrality passed from Behring's Straits and part of Alaska through the States of Iowa, Illinois, Indiana, and Kentucky to the coast of North Carolina. Along a large part of this line observers armed with spectroscopes and cameras were on guard. Of course I do not propose to enter into a discussion of their results here; but, as Mrs. Todd remarks in her valuable book on total solar eclipses, the signal success achieved was Prof. Young's discovery of coronium.

Passing over another period of eighteen years, we come to 1887, when, on the 19th of August, the elaborate preparations made to obtain fresh observations and discoveries were almost completely disappointed by the adverse skies. The shadow on this occasion passed from Berlin across Russia and Siberia to the Japanese Islands. The eastern and the western stations were alike failures, and almost

the only results obtained were some photographs taken in Siberia. On this Prof. Turner has an interesting note in the twelfth volume of the *Observatory*. Let us hope that this eclipse, if we may still use that expression of its so-called return after another eighteen-year period, will be kinder (or rather the clouds less forbidding) on the 30th of August next year. The 'Nautical Almanac' has lost no time in furnishing us with full information respecting the track of the shadow across Spain. My father took part in a famous historic retreat from Burgos; but let us hope that when the astronomers, who will doubtless repair there in the summer of next year, return, they will bring with them a rich harvest of results. At the recurrence of the eclipse eighteen years afterwards, on the 10th of September, 1923, it will not be visible in Europe.

Yours faithfully,

Blackheath, 1904, April 4.

W. T. LYNN.

Meteors from E. of α Cygni.

GENTLEMEN,—

On April 19, 14^h to 15^h , I observed three slow, short meteors from a radiant at $316^\circ + 48^\circ$. I had recorded a few paths from this shower as long ago as 1873 (see 'Ast. Register,' July 1873 and 1874), but had never seen a sufficient number of its meteors in any single year at the April epoch to determine the centre accurately. Combining my observations in different years, there are 10 or 12 paths conformable with this α Cygnid radiant, and a more careful search through my lists would probably reveal some others.

I found a radiant here from a projection of a large number of recorded paths in Weiss's Austrian observations and other catalogues, the centre being derived at $314^\circ + 49^\circ$ from 13 meteors ('Monthly Notices,' vol. xxxviii. p. 396).

Traces of the same shower (or of radiants in identical position) have been detected in March, and I have noticed meteors from it in June, but they come most plentifully in July, and are still prevalent in August and September. My observations in various years have indicated the following radiants:—

1877 March 16-18	$316^\circ + 46^\circ$	Slow.	Suspected.
1873-1904 April 19-25	$316 + 48$	Slow.	12 meteors.
1887 June 12-28	$315 + 48$	Rapid.	6 "
1880 July 2-6	$316 + 46$	Rapid.	5 "
1902 July 9-11	$315 + 48$	Rapid.	3 "
1877 July 11-12	$315 + 48$	Rapid.	8 "
1885 July 13-14	$314 + 47$	V. rapid.	5 "
1898 July 18-28	$314 + 48$	Medium.	6 "
1900 July 18-30	$316 + 48$	Slow.	9 "
1887 July 19-23	$314 + 48$	Rapid.	5 "
1877 Aug. 4-12	$315 + 51$	Rapid.	13 "
1879 Aug. 21-23	$315 + 47$	Slow.	6 "
1876-98 Sept. 15-Oct. 29 ...	$313 + 48$	V. slow.	9 "

The meteors are small with very short flights.

The shower corresponds with no. ccxlv. in my "General Catalogue" ('Memoirs,' vol. liii.), but I believe that two distinct streams are included in the 36 positions there given. There are probably radiants at

$$\begin{array}{rcl} 314^{\circ}.2 + 46^{\circ}.7, & \text{mean of 26 positions.} \\ 311^{\circ}.7 + 51^{\circ}.4, & \text{,, 10 ,,} \end{array}$$

The brighter and swifter meteors are directed from the latter radiant, which is near the small star 51 Cygni.

Bishopston, Bristol,
1904, April 20.

Yours faithfully,
W. F. DENNING.

Eros and the Solar Parallax.

GENTLEMEN,—

Since Mr. Dyson wrote the letter on "Eros and the Solar Parallax" which appeared in your March number, I have had the pleasure of some correspondence with him; and we have come to the conclusion that we are not at present likely to agree upon the matter. The difference between us amounts to this, that I believe in planning the work so as to avoid the introduction into the parallax equations of certain more or less systematic differences between the results from different observatories as they are presented in the form usually adopted; while Mr. Dyson believes that it is unnecessary to insist on this precaution.

It is not unnatural that we should differ thus, because I think the chief and proper use of photographs is to give relative places, while Mr. Dyson contends that they can and should be made to give absolute places, and boldly adds that as long as we confine our attention to relative places we shall not get very far. I am content to defer discussion on this point until we have arrived. Meanwhile, believe me,

Cambridge Observatory,
1904, April 25.

Yours very truly,
ARTHUR R. HINKS.

PUBLICATIONS.

'STARS AND SEXTANTS'*.—Under this title a set of tables has just been compiled by Messrs. Sprigge, Doak, Hudson, and Cox, to facilitate the finding of the "centring error" of sextants by a method suggested by Lord Ellenborough.

This "error" comprises the eccentricity of the axis of the index-bar and the graduated arc, faulty graduation of the arc, and flexure of the frame consequent upon rough usage or variations of temperature. In a first-class instrument, treated with decent care, this error is very small, and is generally entirely neglected;

* J. D. Potter: London, 1903. Price 2s. 6d.

but it may so happen that it is necessary to rate a chronometer by a single set of altitudes, in which case, as any error will be cumulative, a great degree of accuracy is required.

These tables form, then, what the authors term a "help in time of need."

The method is certainly very simple, but it occurs to me that if a navigator is so alive to the necessity of obtaining accurate measurements, and is sufficiently expert to make them and find the error by this method, he would also be keen enough to procure an unexceptional sextant with a Kew Certificate, and then to take care of it, using a coarser one for rough work.

In order to find the "centring error" the observer measures the distance between two stars with his sextant. He then has to "clear the distance" from the effects of refraction as in the "lunar distance" problem. The difference between this distance and the real distance is the sextant error—that is, centring error and index error combined. In order to save all the labour involved in the "clearing," Lord Ellenborough suggests that the pair of stars selected should be on the same vertical circle, in which case the only "clearing" required is to apply the sum or difference of the refractions for the two altitudes.

The tables give a list of pairs of stars at frequent intervals during the year, the actual distance between the pairs, and also the R.A. and Dec. of the poles of the great circles passing through each pair. Now when this pole is on the horizon of a place the pair of stars must be on the same vertical circle. The time at place when this occurs can be found readily, being = R.A. of given pole — Semi-diurnal arc — R.A. Sun. The semi-diurnal arc is given in the tables. The navigator thus knows when to take his observation; and having taken it, together with the approximate altitudes, he obtains the refractions from another table, and adding their sums or differences, according to whether the stars are on the opposite or on the same side of the zenith, he has at once the "cleared distance."

The arrangement and general "get-up" of the tables are excellent, and Lord Ellenborough's introduction is very lucid; but why did not the joint authors put the two Scriptural phrases that occur in the penultimate paragraph of their preface in inverted commas?

C. I.

NOTES.

COMET NOTES.—The paragraph at the end of last month's notes was inaccurate, Brooks' Periodic Comet 1889 V. having been under observation with the 26-inch equatorial at Washington by C. W. Frederick till Feb. 15 last. He obtained observations of it

on 35 days, which is very creditable, considering the faintness of the object and its low altitude.

. On April 16 Mr. Brooks discovered a new comet at Geneva, U.S.A., in R.A. $16^h 58^m$, N. Dec. $44^\circ 10'$; it has a nucleus of mag. 9.5, the combined light being fully 8.5. There is a considerable coma and a fan-shaped tail. At discovery the R.A. was diminishing $2^m 46^s$ daily, and the Declination increasing $38'$ daily. It is thus very well placed for observation, and, owing to the clear weather that prevailed, very numerous observations were obtained soon after the discovery. The following elements are by M. Ebell:—

T	1904 Feb. 28.879	Berlin M.T.
Ω	$50^\circ 53'$	} 1904.0.
ω	275 18	
i	125 0	
$\log q$	0.4295.	

Ephemeris for Berlin Noon.

R.A.					N. Dec.				
h m s					° ' "				
May 2....	16	6	56		52	44		May 18....	14 55 15 57 32
6....	15	50	5		54	23		22....	14 37 10 57 57
10....	15	32	16		55	45		26....	14 20 29 58 1
14....	15	13	50		56	47		30....	14 6 26 57 44

The brightness is steadily diminishing, being 0.81 of that at discovery on May 18.

There have been some amusing mistakes about the comet in the papers; misled by the "Geneva," which they supposed to be in Switzerland, they described the discoverer as Herr von Brooke. A note in *Nature* is also misleading; it describes the comet as the return of Brooks' Comet, instead of the discovery of a new one.

The following are Mr. Seagrave's elements and ephemeris of Encke's Comet, alluded to last month:—

Assumed elements.

Epoch and osculation 1901 July 8.0 Berlin M.T.

M.....	$339^\circ 16' 15''$
π	158 47 57
Ω	334 48 58
i	12 53 38
ϕ	57 49 45
μ	$1073'' \cdot 875$
Period	1206.842 days.

Ephemeris for Greenwich Midnight.

Date.	R.A.	N. Dec.	Date.	R.A.	Dec.
	h m s	°		h m s	°
1904 Oct. 4.....	1 27 18	31 58	1904 Nov. 29.....	20 8 8	10 44 N.
12.....	1 1 39	33 16	Dec. 7.....	19 22 8	2 39 N.
20.....	0 25 31	33 53	15.....	18 36 18	5 45 S.
28.....	23 38 34	33 6	23.....	17 54 45	13 59
Nov. 5.....	22 44 0	30 13	31.....	17 32 44	21 3
13.....	21 48 12	25 5	1905 Jan. 8.....	17 46 43	25 52
21.....	20 56 3	18 23	16.....	18 22 43	27 59 S.

Nearest Earth 1904 Nov. 23; distance 0.374. Nearest Sun 1905 Jan. 4; distance 0.342. Brightest 1904 Dec. 26.

Dr. Berberich gives in *Ast. Nach.* 3940 an ephemeris of Wolf's Periodic Comet, which passes perihelion 1905 May 4, but which may possibly be observed next June, as its brightness will be nearly the same as when last seen in 1899. The elements given are by the late A. Thraen, who computed the perturbations.

Epoch....	1904 June 12.0	Berlin M.T.
M.....	312° 52' 23"	
ω	172 50 38	} 1900.0.
Ω	206 29 0	
i	25 14 40	
ϕ	33 48 59	
μ	520".0519	
log a	0.555973	

Ephemeris for Berlin Midnight.

	R.A.	N. Dec.		R.A.	N. Dec.
1904.	h m s	°	1904.	h m s	°
May 23....	17 55 7	5 14	July 2....	17 22 48	8 42
31....	17 49 48	6 17	10....	17 16 27	8 44
June 8....	17 43 35	7 12	18....	17 11 0	8 32
16....	17 36 45	7 56	26....	17 6 45	8 7
24....	17 29 42	8 26	Aug. 3....	17 3 53	7 32

A. C. D. C.

MINOR PLANET NOTES. — The following new planets are announced:—

Planet.	Date.	Discoverer.	Place.	Mag.
NR	1904 March 20	Wolf.	Heidelberg.	13.0
NT	20	"	"	13.2
NU	1902 March 10	"	"	13.0
NV	1904 April 11	"	"	12.6
NW	12	"	"	12.6
NX	16	"	"	13.0
NY	20	"	"	9.0

The identity of NP with 255 Oppavia is confirmed.

The unlettered planet discovered at Washington 1903 Sept. 28 is not new but identical with 163 Erigone.

NW was moving north at the rapid rate of 23' daily.

NY is unusually bright if really a new planet.

Ast. Journals 559, 560, contain orbits of the planet NF by Messrs. Carrigan and Tillyer. This was at first supposed to be identical with DW, but this was disproved. The eccentricity found is 0.413, which would be the greatest known among the planets. The observations, however, cover too short a time for the elements to be received with confidence.

Dr. E. Strömberg examines in *Ast. Nach.* 3938 the mutual perturbations produced by two minor planets that make a near approach to one another, with special reference to the recent approach of 366 and 386. He finds that in that case the perturbations were probably insignificant, but that some pairs of planets have such nearly identical orbits that their mutual action might be sensible.

A. C. D. C.

OBITUARY.—SIR HENRY THOMPSON. This eminent surgeon died on Monday, April 18, in the eighty-fourth year of his age. His reputation rests mainly on his medical work, but he was successful in many other pursuits which he took up as recreation. Among these hobbies was astronomy, and about the year 1888 he set up a well-equipped observatory at Molesey, evidently intending to devote himself to spectroscopic work, for the chief appliances were a nine-inch object-glass and an objective prism for use with this. He appears, however, to have done little work of this kind, and in 1891 he presented the prism and the object-glass to the Royal Observatory, Greenwich, where the latter is now in daily use for taking sun-pictures. He followed up this gift in 1894 by putting the sum of £5000 at the service of the Astronomer Royal, for the purpose of setting up a large photographic telescope at Greenwich. The practical effect of this is the Thompson Equatorial, a refracting telescope with an object-glass of 26 inches aperture and 22½-feet focus, on a mounting of the German form, set up under the dome which surmounts the new observatory at the southern end of the grounds. Sir Henry always took a personal interest in his gift and in the work done with it, and his presence will be missed at Greenwich on visitation-days, where he has attended regularly for many years.

THE APEX OF THE SUN'S WAY DETERMINED FROM PROPER MOTIONS.—In *A. J.* 558, Prof. G. Comstock has selected 69 stars whose proper motions are well known and which have faint stars near. From the micrometer measures combined with the proper motions he deduces proper motions for the faint stars,

similarly to the work done by Otto Struve in vol. x. of the Poulkova Observations, and more recently by Messrs. Furner and Storey. So far, of course, there is nothing new, but Prof. Comstock uses these proper motions to determine the apex of the Sun's motion in space, and obtains values which he considers very encouraging, viz. R.A. = 297° , Dec. + 28° . Newcomb's determination in *A. J.* 457 is R.A. = $277^{\circ}.5$, Dec. + 35° , and Campbell's spectroscopic result is R.A. = $277^{\circ}.5$, Dec. + 28° .

EROS AND THE SOLAR PARALLAX.—Results of the photographs of Eros taken at the Opposition 1900-01 are coming to hand. *Popular Astronomy* for March last contains a very neat investigation by Mr. H. C. Wilson from 67 photographs taken in October and November 1900, taken at the Goodsell Observatory of Carleton College, Northfield, Minnesota, U.S.A. The photographs were measured with a Repsold measuring micrometer. No réseau was impressed on the plates, but the x and y co-ordinates of stars were measured with the centre of the plate marked by the image of the guiding star or of Eros as origin. Plate-constants were determined by a modification of Prof. Turner's method and the R.A. and declinations of Eros thus determined. These co-ordinates corrected for precession, nutation, aberration, and an approximate value of the parallax of Eros (corresponding to solar parallax $8''.80$) were compared with Prof. Millosevich's ephemeris of the planet. The resulting residuals agreed remarkably with the similar quantities found at Paris and Bordeaux, and seem to be due principally to errors of the elements of Eros' orbit and not to the assumed parallax, so that it appears that from these plates the solar parallax is very near to $8''.80$, probably between that and $8''.81$.

LAPLACE'S NEBULAR HYPOTHESIS.—A paper written by Prof. Stockwell, referred to on page 192 of this number, may be the reason that led Prof. Asaph Hall to publish the following note in the *Astronomical Journal*:—"Laplace is rich in scientific works, and we ought to be fair to him in regard to this hypothesis. In publishing it he says '*I present this hypothesis with the distrust which everything ought to inspire that is not the result of observation and calculation.*'"

THE FLOWER OBSERVATORY.—Since 1896 October the zenith-telescope of this observatory has been energetically used in making observations for determining variation of latitude and the constants of aberration, and the results have from time to time been published. In the *Astronomical Journal* for April 11 another set of determinations of latitude is published, completing the series of seven years' observations originally planned. The value of the aberration constant from this set, which covers the period 1902

Dec. 14 to 1903 Dec. 28, is $20''.524 \pm .0088$. Prof. Doolittle adds: "It is now proposed to continue this work for a time on a somewhat more elaborate plan, giving special attention to the local and diurnal changes which are sometimes noticed. These apparent changes are commonly attributed to anomalous refraction, or temperature effects, which simply amounts to the statement that we are ignorant of their true cause. Whatever this may be, it seems desirable to investigate the subject. Also the wide range found in the different determinations of the aberration constant appears to indicate errors of a systematic character, some of which it should be possible to detect. The present plan is to carry on simultaneous observations with two instruments. The zenith telescope has been renovated and altered, and the second instrument is to be a reflex zenith tube, eight inches in aperture, on the same general plan as that in use at Greenwich.

SUN-SPOTS OF APRIL 1904.—Three fine groups, all large enough to be visible to the naked eye, have been seen on the Sun during the past month. All three were originally of much the same type of formation, and all were about the same distance from the equator. The first of the three groups was first seen on April 8, on the east limb, and passed off at the west limb on April 20. Its heliographic co-ordinates were approximately lat. 16° S. and long. 273° . The group was rather more than 10° in total length, and its major axis was inclined at a considerable angle to the Sun's equator, the leader spot, which was, as usual, much the largest member of the stream, being nearest to the equator. The area of the group averaged about 1000 millionths of the Sun's visible hemisphere. The other two groups appeared together on April 21, both having about the same heliographic longitude, 105° ; the one was in N. latitude 13° , whilst the other was just as far south. The northern group was not now seen for the first time, since a small regular spot had been seen in this region from March 25 to April 5; the southern group appeared on April 21 for the first time. Both groups when first seen were streams of about 12° in length, the leading spot in each being very large and well defined, with a very dark umbra crossed by a network of bright bridges. But before the two groups had reached the central meridian—on April 27—any likeness between them had wholly disappeared. The northern group had stretched out until, on April 25, it was nearly 20° in length, whilst the following spots of the southern group had faded, leaving the leader on April 27 alone but for some faint, hardly-perceptible stains on the photosphere.

THE OBSERVATION OF VARIABLE STARS.—No. 74 of the Harvard 'Circulars' gives some information about observing long period variables, with a list of stars used for the sequences, and asks for observations. But one sentence is especially interesting in con-

nection with some remarks made at the R. A. S. lately on the Rousdon observations *. Prof. Pickering says: "When the measures made at different observatories on the same night are compared, they often differ by half a magnitude or more. A large part of these errors is due to the red colour of many variables of this class, and will depend upon the observer, the size of the telescope, the colour of the star, and other causes." It seems, then, that the residuals shown by the Rousdon observations are not excessive, but that the observations themselves must be very faithfully recorded.

THE WEATHER IN APRIL.—The past month has been, on the whole, decidedly spring-like in character. From April 4 to April 20 the mean daily temperature was above the average every day except two. The mean excess above average on April 12, 13, and 14 was $6^{\circ}.5$. The rainfall for the month is below the average.

GENERAL BASSOT has been appointed Director of Nice Observatory in succession to M. Perrotin. General Bassot, as an officer of the Service Géographique de l'Armée, was in charge of the French determinations of the longitude Paris—Greenwich in 1888 and 1892. He is a member of the Bureau des Longitudes, and was last year President of the International Geodetic Association.

WE are informed that the new Pacific cable has been recently used to determine the differences of longitude between Vancouver and Australia and New Zealand, the measurement of the circuit of the Earth being thus completed. The observations, which were made by two officers of the Canadian Survey Department, are only just completed, so that no results can as yet be announced.

DR. HAROLD JACOBY, formerly Adjunct Professor of Astronomy in Columbia University, New York, has been promoted to the position of Professor, and will continue to be Acting Director of the Observatory *vice* Prof. Rees, who is on leave on account of illness, and will not resume work until July 1905.

AN observatory is being built on the top of Mount Wilson, in California, as a branch of the Yerkes Observatory. It is reported that the equipment will comprise a huge telescope 150 feet long, and of course there will be a spectroheliograph.

DR. W. DOBERCK has returned to Hong Kong Observatory after a period of leave in Europe on account of his health. His assistant, Mr. Figg, is now coming to England for a year or two.

A LOCAL Astronomical Society has been formed at Newcastle-on-Tyne. Mr. T. E. Espin was announced to give the inaugural lecture on March 11. Mr. J. D. Hastings, Warkworth House, Tynemouth, is the honorary secretary.

* See February number, p. 82.

DR. CHARLES LANE POOR, formerly of the Johns Hopkins University, has been appointed to a Professorship of Astronomy in Columbia University.

VISITATION-DAY at Greenwich this year will be on Saturday, June 4.

THE next Meeting of the Royal Astronomical Society will be on Friday, May 13; of the British Astronomical Association on Wednesday, May 25.

FROM AN OXFORD NOTE-BOOK.

THE following extract from a letter sent the other day by Mr. Saunder seems to me of such general interest that I have asked his permission to reproduce it. He has now accumulated enough measures of various points on the lunar surface, on photographs with different librations, to be able to deduce the altitudes above or below the adopted spherical surface of the Moon, and he has compared the altitudes with those found by Franz. The comparison shows a systematic difference as follows, the unit being $\cdot 00001$ of the Moon's radius :—

Crater.	Height.		
	Franz.	Saunder.	F.—S.
Ptolemæus A	+ 51	— 149	+ 200
Mösting A	+ 111	— 56	+ 167
Triesnecker B	+ 196	+ 98	+ 98
Triesnecker A	+ 160	+ 53	+ 107
Bode	+ 136	+ 160	— 24
Bode A	+ 170	+ 25	+ 145
Bode B	+ 148	+ 59	+ 89
Archimedes A	+ 195	— 70	+ 265
Rhæticus A	+ 221	+ 20	+ 201
Herschel c	+ 260	+ 54	+ 206
Mean			+ 145

Concerning this systematic difference of about $1\frac{1}{2}$ miles, Mr. Saunder suggests the following explanation :—“The reason is, I think, this; Franz has determined his plate-constants, including scale and centre, from 8 standard points, all near the limb. We have determined ours from 40 or 50 points—not the same in all cases—scattered over the disc, but with a considerable number near the centre of the disc. Hence we are not necessarily referring to the same mean sphere, and the difference seems to show that our mean sphere has a radius about 1.5 miles larger than that of Franz, which suggests an elongation of the Moon towards the Earth of about 2 miles. Franz's original determination of elongation was just over 1 mile, with probable error nearly 4 miles.”

HERE is a striking description of the Moon of the southern hemisphere, from *Blackwood* for April, p. 485 :—

Not the full Moon of Merrie England with its gnarled familiar faces, but the inverted Moon of the Southern seas shewing the figure of Britannia, erect with trident and attendant lighthouse; for all the world like a British halfpenny shewing blue in the golden setting, to the intense chagrin of every observant foreigner that dare trespass south of the line, so that he who runs may read the portent.

‘THE Reminiscences of an Astronomer,’ by Simon Newcomb (Houghton, Mifflin, & Co., 1903), is a book which does not disappoint the expectations raised by the title, and this is high praise. Those who know anything of the author will expect fearlessly frank and eminently practical views of men and things, and a number of good stories; and these they will certainly find in the book; while, in addition, there are some chapters of early autobiography which will come to most people as a startling surprise. Would anyone have supposed that our great astronomer’s early life was embittered by his obvious deficiencies in skill?—

The skill required on a farm was far above my reach, where efficiency in driving oxen was one of the most valued of accomplishments.

Or that his first scientific appointment was that of assistant to a quack doctor? or that the “most memorable day in his life” was that on which he ran away from the doctor (who seems to have been rather a bad lot), and walked 50 miles to get as far away from him as possible? The author confesses to some hesitation as to the desirability of publishing these records; but I should think few readers will share his doubts. The book is full of good stories, and would provide “copy” for these notes for many months, if one could only be unscrupulous enough to draw upon it. Turning over the leaves almost at random, we come across the very official gentleman who, when introduced to a lady, remained erect and silent because “he had no statement to make to her”: on another page we meet Buffalo Bill as guide to a celebrated geologist: and on another we find a charming little picture of Airy :—

“What place in London interested you most?” said Airy to my wife.

“The first place I went to see was Cavendish Square.”

“What was there in Cavendish Square to interest you?”

“When I was a little girl, my mother once gave me, as a birthday present, a small volume of poems. The first verse in the book was :—

“Little Ann and her mother were walking one day
Through London’s wide city so fair;
And business obliged them to go by the way
That led them through Cavendish Square.”

To our astonishment the Astronomer Royal at once took up the thread :—

“And as they passed by the great house of a lord,
A beautiful chariot there came,
To take some most elegant ladies abroad,
Who straightway got into the same.”

And went on to the end. I do not know which was the more surprised: Airy to find an American woman who was interested in his favorite ballad, or she to find that he could repeat it by heart.

At the end of the book are some good stories of paradoxers, but it gives one's nerves a little shake to find De Morgan's 'Budget of Paradoxes' ascribed to Babbage! (p. 382). Had not Babbage just a drop of paradoxer blood in his own veins? But I am glad to learn of the article by Mr. John Fiske, and shall try to get a copy. An article about "eccentric literature" is likely to have one distinct advantage as compared with an actual collection of it: in the latter there is sure to be an intolerable deal of rubbish, while in the former the few really amusing things have probably been sifted out and brought together. It is, at any rate, my own experience that the writings of paradoxers are often quite dull; and I found the same in a collection kindly sent me the other day—part of a store which the late Prof. Adams had accumulated. But there were some gems. There is a fierce post-card (with a Chippenham post-mark, but nothing else to identify the questioner) reading as follows:—

"A globe demands six Cardinal Points and three axes." *Where are they?*

There is one of our dear old friend Perigal's poems, which I did not know before, beginning:

Of all the many silly notions
Respecting planetary motions,
The very silliest of all
And most unphilosophical
Restricts the Planet's circumvections
To curves derived from Conic Sections!
A conic merely represents
A solid's curved circumference:
And what affinity can be
'Twixt Motion and Solidity?

There is an argumentative letter proving from the Sun's semi-diameter, as given in the *Nautical Almanac*, that his parallax is $8''.939276$; and there is a pamphlet entitled "The Answer to the Universal Question, 'What is an Earthquake?'" which incidentally deals with meteors thus:—

Shooting stars are also issues of magnetic light set at liberty from the metallic bases of the Earth by the Sun's carbon rays. They are creations of disturbance to the laws of obedience and phosphorescent in property.

I do not find, however, any explanation of an earthquake beyond the statement that "as regards earthquakes, expansion of the Earth may occur, and often does, without a quake: and a quake may occur without provoking volcanic eruption."

TALKING of earthquakes, an interesting paper by Prof. Horace Lamb has recently appeared in the *Phil. Trans.*, "On the Propagation of Tremors over the Surface of an Elastic Solid." It has long been known that when an earthquake occurs, disturbances

are propagated to distant places not only along the surface of the Earth, but through the body of the Earth. The latter arrive at any distant station before the former, by an interval depending on the distance; and the observation of this interval enables the observer to infer the distance of the original shock. Prof. Lamb examines how far these experimental results accord with theory; and finds a satisfactory accordance, though there are still some difficulties to be explained, especially those connected with the magnitude of the vertical vibration.

FROM the *Times Literary Supplement* of March 25, I regret to learn that Oxford has been responsible for the production of a book on Dante with the astronomy all wrong:—

For example, there is a celebrated passage in the "Convivio" (II., 2) in which Dante declares that he first saw the "Lady of the Window" when "the planet Venus had twice completed that orbit which makes her appear now as a morning and now as an evening star." Dr. Moore—(by an argument which strikes us as not only unsound, but self-confuting)—reaches the conclusion that the reference here is to the revolution of Venus round the Sun in 225 days, so that the two revolutions will give 450 days (p. 42, and again p. 238). This estimate has been adopted by many writers on Dante, and it lends itself to a very pretty combination with the narrative in the "Vita Nuovo," removing one of the most serious of the alleged inconsistencies between it and the "Convivio." Now this estimate, together with all that is built upon it, has a structural importance in Dr. Moore's whole conception of Dante's mental development; but it is wholly untenable. The period of 225 days was unknown to Dante and his contemporaries, the earliest approximation to it known to the historians being that of Copernicus.

The reviewer goes on to relate how, in 1895, the leading astronomers of Italy were all interrogated on this very point, and unanimously replied that the phrase could only be interpreted as referring to the synodical period of 584 days; but Dr. Moore seems to be ignorant of all this. The reviewer proceeds:—

It is still more disconcerting (though it affects this particular volume only in a minor degree) to find from page 128, compared with page 71 of the author's "Time References," London, 1887, that after seventeen years Dr. Moore has still failed to discover the error of the amazing calculation by which he makes the Sun rise at a quarter past 5 and set at a quarter past 7 in *both* hemispheres, thus giving them each a day of thirteen and a half hours, and making twenty-seven hours in all between sunrise and sunrise at Jerusalem. It is with an effort that we refrain, at this point, from introducing Dr. Moore's favourite typographical form of argument. A strange *obiter dictum* drops from him in quite another connection, to the effect that it would be pushing the demands of scientific accuracy too far to suppose that Dante realized, or intended us to realize, that in Purgatory, in the southern hemisphere, it would be the autumn and not the spring equinox at the time of the vision. Surely we are not to take this as a deliberate defence of the twenty-seven hour day?

THIS criticism of poetry from a scientific standpoint reminds me of an incident related the other day about Milton. A form-master was studying with his boys the fourth book of 'Paradise Lost,' and came to the passage where Satan enters Paradise, "and on the tree of Life . . . sat like a cormorant": whereat one of the boys remarked, "Quite impossible, Sir! a cormorant can't sit on a tree, he's got web-feet!"

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AND COLONIAL GOVERNMENTS.

CATALOGUES ON APPLICATION.

61 STRAND and 4 ROYAL EXCHANGE,
LONDON.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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VOL. XXVII.

JUNE, 1904.

No. 345.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 May 13.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Whittaker. 80 presents have been received since the last Meeting. Those which may be specially mentioned are the first volume of the Greenwich section of the Astrographic Catalogue; Dr. Chandler's revision of the elements of his Third Catalogue of Variable Stars; and the 'Astronomische Jahresbericht,' 1903, presented by the author, Dr. W. F. Wislicenus.

The President. I am sure we are all glad to congratulate the Astronomer Royal upon the appearance of this volume, of which he gave us a preliminary account at the last Meeting; and we will also thank the other donors. To begin this evening's Meeting, I will ask the Secretary to give an account of some papers which we have received but are scarcely suitable for reading in entirety.

Mr. Dyson. *Dr. Downing* sends a short paper in which he compares the fundamental catalogues of Prof. Newcomb and Dr. Auwers. Dr. Downing finds an extremely close agreement of the two systems, showing the extreme accuracy with which the positions of the fundamental stars are known at the present epoch. Then *Mr. Espin* sends a short paper on observations of Double Stars with his 13 $\frac{1}{4}$ -inch refractor. He observed a considerable number of double stars, some of those observed by W. Struve and some by Otto Struve.

Then there are two papers by *Mr. Cowell*, the first "On further Analyses of the Moon's Errors with the Mean Elongation

as Argument." In the second he explains his method of correcting the tabular longitudes used in Airy's reduction.

The Astronomer Royal being called upon by the President to give an account of the micrometer used for measuring photographs of Eros and its results, said :—This micrometer was exhibited at a Meeting of this Society some time ago, and as no printed description was given of it, Mr. Dyson and I thought it might be convenient if we gave an account of it now, and also the results which have been obtained with it in respect to distortion and division errors of the scale, and also of the measurement of photographs of Eros for which it was specially designed. The micrometer is on the same general principle as that devised by Mr. Hinks—that is, a glass diaphragm is used, giving the number of divisions in connection with the micrometer-screw, which gives the fractions of divisions ; but there are a few special features in this instrument which it may be of interest to mention. In the first place, this micrometer was designed to measure photographs taken with two instruments—one the Astrographic, the other the Thompson photographic telescope of double the focal length of the former, which is 13 feet, the Thompson being 26 ; and as the scale of one was twice that of the other, and the same réseau was used on both, it was advisable to have two different magnifying-powers in the body of the microscope, so that we could conveniently measure on both scales. For plates taken with the Astrographic we use a magnification of 2 in the body of the micrometer, and with the Thompson we use the magnification unity, with the same eyepiece in both and with the same screw. There is a convenience in that, and the micrometer is so arranged that by putting in an adapter the change from one power to the other can be made quickly and with certainty. Another feature is that the glass scale in the eyepiece is ruled with double lines, and the réseau lines on the plate under measurement can be made to fall nicely between them, so that a very accurate pointing can be made. These are the two principal ways in which it differs from the plan of Mr. Hinks, and I should say that the general plan of this measuring-machine is similar to that made for the Oxford University Observatory, or, more particularly, to that made for the Perth Observatory, West Australia, and has proved very satisfactory. In the photograph now on the screen it will be seen that the microscope is carried longitudinally in a slide, and that the plate placed on a frame is moved at right angles to the slide by help of counterbalance weights. These weights might be more conveniently arranged, as they are apt to obstruct the light, but that is a small matter. It will be seen that they are of different sizes, so that the plate-holder goes truly along the cylindrical guide without causing any strain. As to the microscope itself, the great point was to get an object-glass with as full a field as possible, so that it would cover 5 millimetres on either side of the centre, and to do this it was necessary to have as long a focus as possible. It was found that an object-

glass of 3 inches focus could be conveniently arranged to allow room for one's knees to be under the table, and the eye at the eyepiece, for that is the practical limitation. Mr. Simms got an object-glass from Germany, and it is, I believe, similar to the one supplied to Sir David Gill for the Cape Observatory. This one has proved very satisfactory on the whole, but there is a certain amount of distortion, which, however, only becomes sensible when you get to the edge of the field. Its effect on the actual reading is very small. This diagram on the screen is a curve showing the distortion, which, it will be seen, is perfectly insensible up to a distance of 15 div. from the centre, and then it increases rather rapidly. There was necessarily a great deal of preliminary work in determining this and such things as the error of scales; but lately we have begun measuring the Eros plates, and it may be of interest if I explain the scheme of measurement. It is arranged to measure some ten or twelve stars with the planet on each plate, and I thought it would be advisable to make a considerable number of measures of Eros compared with those of each star, so we arranged the following scheme:—There are four images of Eros and of the stars on each plate at corners of a square. Two observers measure a plate in succession, each measuring the four images of the planet and two images of the stars—the pair of star-images for the two measurers not being the same, and the plate being rotated through 180° by each, to eliminate personality. They again measure all the four images of the planet and two images of the stars in the same way, so that we get altogether 32 pointings for the planet on each plate. I should explain that on the plates taken with the Astrographic Equatorial we began by choosing stars from M. Lœwy's list, limiting the number to 12 and keeping within 50' from the centre to avoid distortion of the image, and arranged as far as practicable to have these symmetrically about the planet. Mr. Davidson has paid much attention to this point. In addition we have chosen 6 comparison stars, much fainter, of magnitude comparable with that of the planet, all within a radius of 25' from the centre. As the same réseau is used, for both the plates taken with the Thompson and with the Astrographic Equatorial, its scale is different for the two instruments, and a comparison of results is interesting in showing how far we have eliminated errors connected with the réseau. As to the progress of the work of the 144 photographs taken between October 1 and December 31, 1900, with the Astrographic Equatorial, 131 have been completely measured and 10 have been measured by one measurer. Then of 102 photographs taken with the Thompson in the same interval, 55 have been measured. As to the accuracy of the measures, we found, as Mr. Hinks did, that the error of pointing is much less than the error due to the image of Eros, and it is satisfactory to find that the accuracy is abundantly sufficient. Taking a well-defined mark on the plate we find the error of pointing is $0''.02$, but the probable

error of a pointing on the image of the planet is $0''.067$ on the Astrographic plates and $0''.049$ on those taken with the Thompson; so that there is a very marked gain in using the larger telescope. We have worked up results for October 26 and 27, and formed corrections to the ephemeris of Eros. With the comparatively large parallax factor between the observations of the evening and the next morning we ought to get a value of the solar parallax correct to nearly one-hundredth of a second of arc. A rough reduction of the result on these two nights has been made, which gives a value of the solar parallax not greatly different from the present adopted value; I give this quite as provisional, as the work has been done rather hurriedly, but it is illustrative of the accuracy we may hope for in the combination of results. The accuracy shown is much of the same order as M. Lœwy and Mr. Hinks have arrived at, and it is satisfactory to know that our photographs give results comparable with those taken elsewhere; and from the totality of a large number of photographs taken at different places we shall certainly get an extremely accurate value of the parallax.

The President. Under the guise of a simple description, the Astronomer Royal has provided us with some important information with regard to a very important problem, and there are many, no doubt, who would like to make some remarks on this very interesting paper.

Sir David Gill. I would rather listen, in the first place, to the remarks of Mr. Hinks, because my own experience of the solar parallax is not related to the conditions before us. I can tell you that, when I saw results of such accuracy, it was with no small measure of relief that I saw the main result was not very far from that which we derived from the minor planets. With that result I am, perhaps, satisfied enough as to say that it is a very good account of the Greenwich photographic observations. I mean to say that a man who starts on a new job of this kind generalizes in the first place on some conclusions which are extremely satisfactory, and as he gets further on with the work he finds something which drives him to the verge of despair, and I think Mr. Hinks, when he comes to relate some of his experiences, will be able to tell you some story of that kind. I have had opportunities of seeing Mr. Christie's measurements of a number of plates, and I was very much astonished to find the extreme accuracy of the result that came out. There is no doubt that photography, if properly done and properly applied, and freed from all possible kinds of errors which may affect it, is capable of giving good results. But between the realization of that end and between the trials with which the operations are begun there are vast difficulties. I hope and I believe that at Greenwich they have been fortunate in avoiding many, but I know, from what I have heard at other places, that there are large systematic errors which exist, and which have to be dealt

with very gingerly, before proper conclusions can be arrived at. I can only repeat my great satisfaction at what I have seen of the results of the instruments at Greenwich, and I feel perfectly certain that with such means we have a tremendous grip of the solar parallax within our hands.

Mr. Hinks. I am not prepared to unfold my tale of woe this evening. As Sir David has told you, I have been coming across some difficulties in my own work in connection with the solar parallax problem, which turn up when you combine the results of eight or ten observatories. Each observatory is often self-consistent, but when one begins to combine observations taken at different places, one finds systematic discrepancies, which are, in some cases, very puzzling. I am fortunately getting near the end of the work, and I may say, from experience of these observations which I have in hand, that it seems to me the work they are turning out at Greenwich is about on a level with the best of work I have had to deal with. What gives me trouble now is just the worst, and I shall be glad not to say anything about the matter until a later date.

Prof. Rambaut. I should like to ask the Astronomer Royal one or two questions, but before doing so I should like to express the pleasure with which I have listened to this most interesting paper. It is not my intention to refer to the measures of the solar parallax, but to other parts of the paper. The figures put on the board seem to me eminently satisfactory, and even in the face of the warning of Sir David Gill, I think I would advise the Astronomer Royal, if I may take the liberty in doing so, not to be too much depressed as to restrain his remarkable activity in obtaining accuracy in this important work. The question I should like to ask the Astronomer Royal refers more particularly to the first part of the paper, the description of this beautiful instrument. It chiefly interests me just now, for I am looking forward to the pleasure of examining the screws and scale of the new instrument which I have just purchased; and the first question I would ask is with regard to the comparative probable error of measures made with the Astrographic and the Thompson equatorials. It seems from the figures on the board that these are almost exactly in inverse proportion to the focal length, and the point I would like to refer to the Astronomer Royal is whether the errors due to the images on the two series of plates are comparable with the size of the telescope employed. With regard to the instrument itself, I was struck with the form of the scale; and with a view to any possible difficulty which may be lying in wait for me, I should like to ask him whether the scale is etched or ruled. Lastly, with regard to the question of distortion. That is a question which I have not yet given very serious attention to. I have been warned of it, and am prepared to deal with it, but I should like to know, if possible, what is the best practical method of getting rid of it.

I think that is a very important matter, and must be of interest to all persons engaged in such work.

Mr. Dyson. Perhaps I might add one or two words to what the Astronomer Royal has said. In the micrometer we have an extremely good instrument, and any errors which are shown by these plates are not errors of measurement, but are inherent in the star-images. To make the micrometer a good instrument after it came from the makers, there was really a great deal of work to do in the determination of the errors of screws and scales, and that work was done largely by Mr. Davidson, and it is largely due to him that the instrument is as it is. In the end I think we have an almost perfect micrometer. With regard to the accuracy of the results, we are very well pleased to find that the results from the two nights already reduced comes nearly to the present adopted value of the solar parallax. I do not imagine that these two nights are exceptional, in fact I expected *a priori* that the plates on one of the nights would have given a discordant result, and I am surprised to find the results obtained.

Sir D. Gill. When the Astronomer Royal says that the lens used is the same as the lens supplied to me, I presume that he means the lens supplied to my transit-circle microscopes, but those do not cover so large a field.

The Astronomer Royal. As to what Dr. Rambaut said first with regard to the relative accuracy of the plates taken with the Thompson and with the Astrographic. As I have said, and as Mr. Dyson has mentioned, the large probable error of the Astrographic plates is, I think, probably rather largely exaggerated by bad definition of the photographs of October 26. If we take October 27, the relative accuracy would be more in the ratio 2 : 1. With regard to the point he raised as to the images with the Thompson, I think our impression is that there is not so much gained in the star-images as in the réseau lines. Having double the scale, the réseau lines are better defined—that is to say, we use half the magnification of them, and that is where we gain with the longer focus. The determination of distortion was made by using a scale as a standard of length, and this was moved about to different parts of the field so as to compare different intervals with the standard which was placed in the plane of the plate. The scale in the eyepiece of the telescope was made by Mr. Simms on his dividing engine.

Mr. J. Franklin-Adams gave an account of his recent work at the Cape Observatory, where he has been staying during the past nine months to complete the southern portion of a Photographic Atlas of the Heavens on the scale of Argelander's maps, *i. e.* 20 mm. to one degree. This Atlas is specially designed to facilitate research into the structure of the Milky Way.

The lenses used were a 10-inch lens of short focus, specially designed by Mr. Dennis Taylor, of the firm of Cooke & Son, which gave a field 15 inches by 15 inches, and a 6-inch lens

giving a field 12 by 10 inches, used for taking duplicate plates. The scheme of the Atlas divided the whole sphere into 206 fields, 115 of which, covering the sky from declination $7^{\circ} 30'$ N. to the South Pole, were to be taken at the Cape. These 115 fields were taken with the 10-inch lens with two hours exposure, with the 6-inch again with two hours exposure, and then again with the 10-inch, three exposures being given, each of seven minutes, the stars being displaced so as to fall at the angles of small triangles. Mr. Franklin-Adams spoke enthusiastically of the driving-clock of his instrument, some plates having been exposed for the whole two hours without once touching the motor. He exhibited some photographs of his instrument and of his results, and said that at present he only proposed to reproduce a few copies, but these, whether photographic or mechanical, will be published absolutely untouched.

The President. We have heard with great interest of this enterprise which one of our fellow astronomers is conducting, and following the examples before him. I am sorry to think that one of the reasons which Mr. Adams had for choosing the Southern Hemisphere first was that he was driven there by bad health. I mention that because I wish to express our gratification at seeing him thoroughly restored, so that he can now work in the Northern Hemisphere. I will ask Sir David Gill to make a few remarks.

Sir D. Gill. When Mr. Franklin-Adams's telescope arrived, I was filled with admiration at the extremely satisfactory way in which it was adapted for the special purpose he had in view. When Mr. Franklin-Adams himself arrived, I do not think I ever saw a man so perfectly unadapted for the work he had undertaken. He could hardly move one leg before the other, but he managed to lie upon his back and get to his chair, and there he worked in that condition, when most men would have kept in bed. I told him if he went out to Caledon he might get cured of his rheumatism, and he finally turned out a remarkably hale man, and managed to do a remarkably good piece of work. I think he deserves extraordinary credit for the ingenuity of his preliminary arrangements, for his tremendous energy and persistence in carrying on this work. Those who are going to the Royal Society's *Conversazione* to-night will see the photographs themselves, and they will find they are well worth seeing.

Dr. Rambaut. I think Mr. Franklin-Adams is to be congratulated not only upon this remarkable work, but upon his wonderful cure. I was very much struck with the running of the control of the clock, of which he spoke so enthusiastically, and I am anxious to know whether he would recommend the use of this kind of clock for a telescope of longer focal length. I should like to know whether he had to adjust the rate for the particular declination under observation so as to take out the changes of refraction, or was it a happy chance that it was so remarkably exact? There is no doubt about it, that the clock must be a very accurate one.

Lord Rosse. It seems to me to be quite a new thing to be able to photograph without touching the clock. I understood that with a free-going clock it was always necessary to watch it. A friend of mine told me lately that he never trusted his clock, although every fortnight or so he went over it and oiled it so as to give it a good chance. Many photographs of a large field always show considerable distortion in the images of the stars towards the edges of the plate. I was always under the impression it was theoretically impossible to get a lens that would show round images towards the edge of the plate, but those plates taken by Mr. Franklin-Adams which I have examined with my pocket-lens show such images very round.

Mr. C. Thwaites. Will Mr. Adams say what form of governor is used and what form of bearings? Were plane bearings or ball bearings used? That might have a great effect upon the driving of the clock.

Mr. Franklin-Adams. In answer to Dr. Rambaut, I may say that I think the Repsold driving-clock is quite the best arrangement that I have ever seen or tried. It will stand a drag or an accelerating force of quite 30 lbs.—a force quite enough to stop most driving-clocks. A transparency will be exhibited at the Royal Society rooms to-night which has been taken from a negative exposed for two hours without any hand-grinding or control from any other clock. With reference to Lord Rosse's remarks about the definition of the corners of so large a plate, I would refer him to Mr. Dennis Taylor's Appendix A which will be printed with my paper. I am asked whether the clock would be satisfactory for a telescope of long focus; and as to this I must say that though the camera gives pictures on only a small scale, yet the findings—telescopes are 6 feet long and the object-glasses of 6 inches diameter, and with a power of 250 give most beautiful images of the key-maps. The good running of the instrument is primarily due to the perfect adjustment and balance of the instrument. The polar axis at both ends turns on ball-bearings.

Sir David Gill. The bearings of the telescope are not in a strict sense ball-bearings, the axis rests upon fixed bearings, but is relieved by ball-bearings.

Dr. Rambaut. Mr. Adams mentioned that he uses electric signals. I think this method has been previously adopted at various places—the Radcliffe Observatory for one—for motion in R.A., but I am not aware that it has been used for motion in declination.

Sir David Gill. It was suggested in an article of mine.

The President. I am very glad we have had Sir David Gill with us to-night to explain the trying circumstances under which the work has been done. I am sure it will increase our thanks to Mr. Franklin-Adams for his interesting paper.

Mr. Bellamy showed and described photographs of a cluster of stars in Cygnus not hitherto recorded in the catalogues.

Sir David Gill being called upon by the President to give some account of the new instruments at the Cape Observatory, said that he preferred to defer this as time was limited; and the ballot was taken at a quarter to seven.

The following papers were announced and partly read:—

J. Franklin-Adams. "Milky-Way Charts of the Heavens to Argelander's Scale $1^{\circ} = 20$ mm.; with Description by H. Dennis Taylor and Alfred Taylor of the Lenses and Mount."

P. H. Cowell. "Methods of correcting Moon's Tabular Longitude."

A. M. W. Downing. "The Definitive Places of the Standard Stars for the Northern Zones of the *Astronomische Gesellschaft*."

F. W. Dyson. "Note on the Formulæ connecting 'Standard Coordinates' with right Ascension and Declination."

A. E. Conrady. "Note on a suggested Method of Determining the Declination of Stars."

P. H. Cowell. "Further Analyses of the Moon's Errors, with the Mean Elongation as Argument."

Royal Observatory, Greenwich. "On the new Greenwich Micro-meter for Measurement of Photographs of Eros."

H. C. Plummer. "Note on the Influence of the Plate-Constants on the Accuracy of the Position of an Object measured on a Photograph."

F. A. Bellamy. "A new Cluster in Cygnus, with R.A. and Decl. and 103 Stars included in it."

Rev. T. E. Espin. "Micrometrical Measures of Double Stars made with the 17 $\frac{1}{4}$ -inch Reflector. Second Series."

F. W. Dyson. "Note on the Instrumental Errors affecting Observations of the Moon."

The following gentlemen were elected Fellows of the Society:—

Zia Mddin Ahmad, B.A., Trinity College, Cambridge.

Daniel Buckney, 61 Strand, W.C.

Robert James Wallace, Yerkes Observatory, Williams Bay, Wis., U.S.A.

The following Candidates were proposed for election as Fellows of the Society:—

Major C. F. Close, C.M.G., R.E., Brompton Barracks, Chatham (proposed by Sir David Gill).

Thomas Andrew Common, 63 Eaton Rise, Ealing, W. (proposed by W. H. M. Christie).

MEETING OF THE BRITISH ASTRONOMICAL
ASSOCIATION.

Wednesday, 1904 April 27.

President: S. A. SAUNDER, M.A., in the Chair.*Secretaries*: A. C. D. CROMMELIN, B.A.
J. G. PETRIE.

Mr. J. G. Petrie read the Minutes of the previous Meeting, which were confirmed.

Mr. A. C. D. Crommelin read the list of presents received, and the thanks of the Meeting were accorded to the respective donors.

The name of one Candidate for Membership was read and passed for suspension, and the election by the Council of three new members was unanimously confirmed.

Mr. Crommelin read the Interim Report of the Variable Star Section, by the Director, *Colonel Markwick*.

The President said there was only one unsatisfactory feature in the Report just read, and that was the falling off in the number of observers. There was no branch of work which offered more opportunity to the amateur astronomer. Much work could be done with the naked eye and more with a pair of binoculars, still more of course with a telescope—it did not matter of what size.

The Rev. T. E. R. Phillips read the Report of the Jupiter Section, 1901–02.

Mr. E. J. Neubegin read a paper on “Sun-spots of April 1904,” illustrated by several lantern-slides which were thrown upon the screen.

The President said that about two months ago a question was asked as to the smallest spectroscope that could be employed for detecting prominences on the Sun’s limb. A few weeks ago one of the members, *Mr. G. F. Pollock*, showed him a 3-inch pocket-spectroscope, through which he said he had seen the spectra of prominences on the limb. He (the President) had tried the instrument on his own telescope—a 7-inch refractor, and, working round the limb, in a very short time picked up quite distinctly a couple of prominences. Others he thought would be pleased to learn that with a small pocket-spectroscope, which could be attached to their own telescopes, they could observe sections or prominences.

Mrs. Herkless thought the close proximity of the spots to the prominence was a striking and interesting fact.

Mr. A. J. S. Adams asked if there were other cases of apparent-coincidence between spots and prominences.

Mr. Neubegin said this was his first experience of such a coincidence, and he did not like to say that one was responsible for the other.

Mrs. Herkless said that *Father Secchi* in ‘*Le Soleil*’ gave an account of at least one such case.

Mr. E. W. Maunder said the connection between special sun-spots and prominences had been observed a good many times. There was a very striking instance in the eclipse of 1901. There was just one great spot visible in that year, and that happened to be on the very limb of the Sun at that date; and the photographs taken during the eclipse showed a couple of fine prominences just rising over to the very region of the spot. There were two very fine groups of spots on the Sun at the present time. They came on early on the 21st April. On the 23rd both groups had been distinctly seen with the naked eye without any optical assistance at all.

Mr. C. Thwaites said that he has been able to see the two groups referred to by Mr. Maunder with the naked eye.

Rev. T. E. R. Phillips asked whether there was any record of sun-spots having been seen before the invention of the telescope. He believed it was not the case.

Mr. Crommelin said that in a French publication (he thought it was the *Bulletin Astronomique*), two or three months ago, there was a record, taken from a Chinese source, of what professed to be sun-spots, dating back some 500 or 600 years before the invention of the telescope. The compiler of the note even tried to get the 11-year period out of them, and found there was some evidence for it although not very strong. Of course the records were rather scattered, but there seemed to be no doubt, judging from the translation, that they were really records of sun-spots taken from Chinese sources.

Mr. W. T. Lynn said he had met with several records of sun-spots having been seen before the invention of the telescope. There was one recorded in the time of Charlemagne (A.D. 807), and he thought other instances could be found. The one in question was thought at the time to be a planet, but as it was noticed on the disc during seven days, it must have been a large spot.

A paper by *Mr. W. H. S. Monck*, on "The Sun's Motion in Space," was next read. The writer contended that if the stars selected for comparison were numerous, their motions well ascertained, and the selection impartial, there was little chance of material error. There was, however, one consideration to which sufficient attention had not been paid. A great part of the stars belonged to the galactic ring, which was probably revolving, and there was the danger of confounding the revolution of this ring with the motion of the Sun. It might be that an impartial examination of stars situated at all parts of the ring would exclude such an error, but the proper motions of northern stars were much better known than southern ones, hence northern stars usually entered more largely into computations of the Sun's motion than southern stars.

Mr. Crommelin thought Mr. Monck was certainly right in giving his warning that they must see that the stars used were distributed

over the whole of the heavens impartially, because, if not, any common drift, such as the rotation of the Milky Way, would undoubtedly affect the result; but if they took them over the whole sky, they were free from any error that that would introduce. Of course, the rotational movement being in the same direction all the way round, it was obviously different in its effects from the translational movement, which had opposite effects in the apparent motion it introduced on the two sides of the apex. There might also be something in what he said about the deficiency in the known proper motions of the fainter southern stars; but if that were so, that uncertainty would soon be wiped out now that the southern heavens were being surveyed with just as much accuracy as the northern ones. It was certainly curious that different groups of stars gave very different positions for the solar apex. It looked as though accidental errors were not entirely eliminated, and they could not have any great confidence in the solar apex, as now given, within four or five degrees and perhaps more.

Mrs. Herkless read a paper on "The Bolometer and the New Spectrum."

Mr. H. Ellis asked whether he was right in believing that the heat-rays were subject to the same law as light-rays. In a visual part of the spectrum light was supposed to interfere with light to produce the dark space seen in the spectroscope. *Mrs. Herkless* spoke of a dark space in the invisible red end of the spectrum. Did heat interfere with heat in the same way in which light interfered with light?

The President thought that the dark spaces were caused by absorption, not by what was known as interference.

Mr. Crommelin remarked that *Mrs. Herkless* had given them a very interesting account of the new spectrum. The reason, perhaps, that it had not come more to the front was that it was so very difficult to interpret this spectrum. As yet they had merely got a number of dark bands, but it was not known to what substances they were due. With regard to what had been said about the interference of light and heat, he fancied that in nature all these radiations were the same thing; it was merely that our different senses were limited to different parts of the complete energy-spectrum. They all consisted of vibrations of various lengths in the ether.

Mr. Maunder said it was well known that the lines of the spectrum of helium were composed of six series, each series of which was in perfect rhythm. These series were ranged, further, in two sets, and the first line of each of the two principal series lay in the infra-red. The place of each of these lines was predicted theoretically, and the line itself was detected by Profs. Runge and Paschen, as nearly as might be in the theoretical place by means of the bolometer, thus showing that the nature of the lines throughout was precisely the same.

Mrs. Herkless asked if the nature of the lines of the spectrum

from end to end was the same—that is, if they proceeded from the same radiant-energy: to which Mr. E. W. Maunder replied in the affirmative.

Mr. Crommelin asked if it was possible to photograph lines in the infra-red from helium.

Mr. Maunder replied that Runge and Paschen were able to detect the first line of each series by means of the bolometer, not by a direct photograph.

Mr. Crommelin. Not very far down, I suppose?

Mr. Maunder. A good way down. In one case the line was at 11,000 tenth-metres; in the other 20,000 tenth-metres, which was a good way below the limit of the visible spectrum, which ended about 7800.

The President said there was absolutely no reason for supposing that what Prof. Langley called the dark lines in the infra-red were, in their nature, in the slightest degree different from those seen as dark lines in the visual part of the spectrum, or which were photographed as dark lines in the ultra-violet part.

Mr. W. T. Lynn read a paper on "The Return of Wolf's Periodic Comet." The writer said that, although this comet was not due to return to perihelion until early in May next year, Prof. Berberich thought it possible that it might be seen when near opposition to the Sun during the approaching summer, and had accordingly published an ephemeris from elements computed by the late Pfarrer Thraen.

The President said they all knew that a new comet had recently been discovered by Mr. Brooks at Geneva, U.S.A.

Mr. Maunder did not think there was much to be said about Brooks' Comet. It was discovered on the evening of April 16. Its place at 9.50 Geneva M.T. on April 16 was R.A. $16^{\text{h}} 58^{\text{m}}$ and $44^{\circ} 10' \text{ N.}$ Dec., and Prof. Pickering had since found it on a number of his plates, taken night after night, of the entire heavens. The computed orbit was a rather extraordinary one, giving a period of only three years and small eccentricity.

Mr. Crommelin said when he first saw the orbit from Harvard it excited his incredulity—a nearly circular orbit, in a retrograde direction and very nearly at right angles to the ecliptic, and with a period of three years, every one would admit was a very improbable one. He, Mr. Crommelin, had tried his hand at computing the orbit, but he would not give the results as there was some error in them, but he thought a parabolic orbit would fit the observations.

Mr. E. Holmes read a paper, accompanied by lantern-illustrations, entitled "A Telescope-house for about £3."

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

THE usual monthly Meeting was held on the afternoon of Wednesday, May 18, at the Society's Rooms, 70 Victoria Street, Westminster, *Capt. D. Wilson-Barker, F.R.S.E.*, President, in the Chair.

The Hon. F. A. Rollo Russell read a paper on "The principal Causes of Rain," in which he stated that the chief causes of rain are only four, but several of these are often in co-operation. These causes may be briefly described as follows:—(1) The forced ascent of moist air by the slopes of mountains; (2) a mass of air invading rather suddenly another mass moving from an opposite direction, and maintaining its flow below the opposing current which it displaces; (3) the ascent of more or less moist air through heavier and colder air to a height where condensation of vapour takes place, increased radiation of heat towards space and often electrical developments producing further condensation, increase of temperature, and renewed ascent with the same results; (4) the mixture of currents of air from different directions.

A paper by *Mr. W. C. Nash*, on "The Observations of Rainfall at the Royal Observatory, Greenwich, in the Years 1815 to 1903," was also read. The author has made a full inquiry into the circumstances relating to the early history of the register, and has drawn up an authoritative table of rainfall for the long period of 89 years. The average annual rainfall is 24·36 inches, and the number of rainy days 157. The greatest fall was 35·54 inches in 1903, and the least fall 16·38 inches in 1858. During the five months January to May no monthly fall exceeding 4·37 inches was recorded; but in the remaining seven months there were 24 falls exceeding 5 inches. Light falls of rain are spread principally through the nine months January to September, with a decided preponderance in spring.

The Duration of the Perseid Shower.

To meteoric observers the August Perseids form the most interesting shower of the year, for it is regularly visible during a long period in the summer season, when the nights are often clear and the temperature agreeable. The particular dates marking the opening and closing of the display are not definitely known, but the shower is certainly in fair activity on July 19 and August 16, including an interval of 29 nights. But I think the first feeble traces of it are sometimes evident as early as July 7, while lingering signs are still apparent as late as August 21 and just possibly on August 25, a period of 50 nights. A few swift meteors, with the usual streaks, were seen during the years 1888–1902 giving a well-defined radiant at $10^{\circ} + 45\frac{1}{2}^{\circ}$, which nearly agrees with Kleiber's

computed position at $9^{\circ} + 46^{\circ}$ for July 8. Though the accordance is very suggestive, the meteors seen at Bristol may not possibly have been true Perseids. Traces of the shower have also been observed here on July 11, 12, 13, &c., and the following radiant determinations have been made of these vanguards of the stream:—

1877-1902	July 7-9		$10^{\circ} + 45\frac{1}{2}^{\circ}$	6 meteors.
1877-1888	„ 11		$11^{\circ} + 48^{\circ}$	5 „
1877-1888	„ 12		$13^{\circ} + 47^{\circ}$	5 „
1876-1902	„ 16		$14^{\circ} + 54^{\circ}$	7 „
1876-1900	„ 18		$17^{\circ} + 50^{\circ}$	5 „
1876-1901	„ 19		$19^{\circ} + 51^{\circ}$	7 „

There are minor showers at the following points:—

$7^{\circ} + 35^{\circ}$		$30^{\circ} + 36^{\circ}$
$21^{\circ} + 57^{\circ}$		$32^{\circ} + 53^{\circ}$
$23^{\circ} + 43^{\circ}$		$34^{\circ} + 47^{\circ}$

With regard to the closing stages of the shower, the radiants derived here have been as under:—

1876-1901	August 16		$53^{\circ} + 58^{\circ}$	19 meteors.
1885-1901	„ 17		$54^{\circ} + 60^{\circ}$	5 „
1885-1901	„ 18		$55^{\circ} + 59^{\circ}$	11 „
1884-1901	„ 19		$57^{\circ} + 59^{\circ}$	5 „
1885-1901	„ 20		$57^{\circ} + 59^{\circ}$	10 „
1887-1903	„ 21		$64^{\circ} + 59^{\circ}$	12 „

There are outlying and apparently distinct radiants at:—

$48^{\circ} + 43^{\circ}$		$61^{\circ} + 37^{\circ}$
$51^{\circ} + 63^{\circ}$		$70^{\circ} + 67^{\circ}$
$60^{\circ} + 49^{\circ}$		$72^{\circ} + 52^{\circ}$
$60^{\circ} + 59^{\circ}$		$77^{\circ} + 57^{\circ}$

The shower at $60^{\circ} + 59^{\circ}$ in Camelopardalis corresponds nearly in position with that of the Perseid radiant on August 21, and for this reason it is impossible to fix the absolutely final date for true Perseids. Though, as already stated, there is slender evidence of the shower's prolongation to August 25, I have never recorded a sufficient number of its meteors to obtain the radiant satisfactorily after August 21.

This year moonlight will not interfere with observations of the earlier part of the shower (from about July 8 to 19), nor with the maximum and concluding phase (from about August 6 to 19), so it is to be hoped that a number of new observations will be obtained.

Bishopston, Bristol,
1904, May 5.

W. F. DENNING.

The actual Distance between Two Stars.

WHEN the distances of two stars from the Earth have been accurately determined, the actual distance between the stars themselves can be easily computed by simple trigonometry. Take the case of Sirius and Procyon. The parallax of Sirius is $0''.37$, and that of Procyon $0''.325$; and the apparent angular distance between the two stars is about $25^\circ 43'$. From these data I find that the actual distance between Sirius and Procyon is almost exactly half the distance which separates Sirius from the Earth. From this it follows that the parallax of Procyon seen from Sirius or of Sirius seen from Procyon is about $0''.74$. Hence the light of Sirius seen from Procyon will be increased 4 times or about 1.5 magnitude, and its magnitude would therefore be $-1.58 - 1.50 = -3.08$ magnitude. The light of Procyon, as seen from Sirius, would be increased in the ratio of $\left(\frac{0.74}{0.325}\right)^2$, or 5.185 times.

This corresponds to an increase of 1.78 magnitude. Hence the magnitude of Procyon seen from the Sirian system would be $0.48 - 1.78 = -1.30$, or nearly as bright as Sirius appears to us.

Again, take the case of η and μ Cassiopeïæ, which are at an apparent distance of about $3^\circ 50'$. The parallax of each is about $0''.20$. From these data I find that the actual distance between the two stars would be only $\frac{1}{15}$ of their distance from the Earth, and hence the parallax of each as seen from the other would be about 3 seconds of arc. Hence the apparent brilliancy of each would be increased 225 times (15^2), or about 5.88 magnitudes. As the photometric magnitude of η Cassiopeïæ is 3.64, it would therefore shine as a star of -2.24 as seen from μ ; and the magnitude of μ being 5.22, it would appear as a star of -0.66 magnitude as seen from η . But in this case the parallaxes are not so certain as in the case of Sirius and Procyon.

The double stars are of course much closer. In the case of 61 Cygni, if we assume that both components have the same parallax, about $0''.4$, their distance apart being $22''$ (1900), we have their actual distance 55 times the Sun's distance from the Earth, and their distance from the Earth 515,662 times the same unit. Hence the light of each as seen from the other will be increased in the ratio of $\left(\frac{515,662}{55}\right)^2$ or 87,913,200 times. This corresponds to 19.86 stellar magnitudes. Taking the magnitudes of the components at 5.3 and 5.9, we have their brightness increased to -14.56 and -13.96 magnitude respectively, or brighter than full moonlight on the Earth. I have seen it stated somewhere that the components of α Centauri would only shine as bright stars as seen from each other; but this is not correct. The parallax being $0''.75$, and the mean distance between them being 23 times the Sun's distance from the Earth, I find that their light

would be increased by 19·7 magnitudes; and as their photometric magnitudes are, according to the Harvard measures, 0·36 and 1·61, their brightness would be increased to -19·34 and -18·09, magnitude respectively. In fact, each, as seen from the other, would shine as a small Sun. As, however, the eccentricity of the orbit is considerable (about 0·528), this brightness would vary to some extent. But even at their farthest distance apart they would still shine as small Suns as seen from each other. But of course they would show no perceptible disc without optical assistance.

J. E. GORE.

International Research in Terrestrial Magnetism.

IN 1902 January, Prof. Bauer submitted to the Carnegie Institution a plan for a Bureau of International Research in Terrestrial Magnetism, "to investigate such problems, of world-wide interest, as relate to the magnetic and electric condition of the Earth and its atmosphere, not specifically the subject of inquiry of any one country, but of international concern and benefit." His programme included:—

- (a) A magnetic survey of unexplored regions and of ocean areas (which, since the advent of iron ships, have been almost entirely neglected).
- (b) International observations of the variations, so as to secure uniformity in observation and reduction, and insure prompt co-ordination and publication of the results; also establishment of a network of secular-variation stations throughout the accessible parts of the globe, in order to keep magnetic maps ever up to date in all regions.
- (c) Observations in ocean depths and atmospheric regions, and other problems, such as the correlation of disturbances with geological features or with meteorological phenomena.

In 1903 December the Trustees of the Carnegie Institution took the necessary steps to carry these suggestions into effect, allotting \$20,000 as a first annual grant, to be continued, if the scheme proves successful, until the completion of Prof. Bauer's programme.

In 1904 February the Executive Committee appointed Prof. Bauer Director of the new Department, and proposed to set aside one-half of the grant for office expenses (reduction and discussion of existing observations), and the other for observational and experimental work, with power to accumulate for any large undertaking by reserving a portion annually.

To the chorus of emphatic approval which has already greeted this proposal we can now add our hearty congratulations to Mr. Carnegie on the sagacity of the Trustees, to Prof. Bauer on

the decision to sanction his cherished scheme, and to the world on the exceedingly valuable results that may be confidently expected. There has long been a crying need for the establishment of some authority to deal with the mass of undigested observations that have been steadily accumulating for many years at an increasing rate, and which formed the burden of a recent Presidential Address to the British Association, although this is, perhaps, felt even more in regard to meteorology than to magnetism. But the new scheme provides the thin end of the wedge in the correlation of phenomena, and it is but a step from that to the appointment of a new or perhaps subsidiary department to deal with Meteorology more directly.

The keeping up to date of magnetic maps will be a long step in advance, as, even in England, the secular variations cannot be said to be accurately known. We shall also be much interested in the methods to be devised for magnetic observations under the sea (balloon observations do not now appear quite such a novelty), and in the evolution of fresh fields of research which must necessarily follow the successful establishment of the new department.

W. W. B.

Some interesting Double Stars.

[Continued from vol. xxvi. p. 99.]

Σ 554. R.A. $4^h 24^m 26^s$ } Mags. 6.5. Colour: yellow.
N.P.D. $74^\circ 34'$ } 9.0.

The principal star is 80 Tauri and has, according to Auwers, a proper motion of

+0".072 in R.A. and +0".004 in N.P.D.,

which would since 1831 have so separated the stars that the distance would be over 5". They are now so close that the largest instruments fail to separate them. Hence there is strong evidence of physical connection. When, however, the motion is more closely considered there is no room for doubt, for from

1831 to 1858	there is an annual motion of	0".015,
37 "	71 "	0".021,
58 "	78 "	0".035.

The pair is evidently binary, the plane of the orbit being nearly coincident with the plane of the line of sight.

Flammarion remarks that the smaller star is "vapoureuse."

Σ 948. A triple system of which the principal star is 12 Lyncis, R.A. $6^h 37^m 4$ and N.P.D. $30^\circ 28'$. The two largest stars are 5.2 and 6.1 magnitude respectively, and both are yellow. Sir W. Herschel discovered the system in 1782, but did not make a

d measure of distance, which is seen, from the means given below, to
 y have remained constant for the last 70 years. An annual decrease
 in angle of $0^{\circ}523$ satisfies these means, as will be seen in the
 d column O—C, which represents the difference in the observed
 s place and the computed. Assuming a circular orbit, the period
 t would be about 700 years.

			O—C.
1782.....	181°	..	+1°5
1823.....	158	..	—0°8
1833.....	153	1"62	—0°5
1844.....	157	1°61	—0°6
1856.....	141	1°63	+0°1
1866.....	136	1°69	+0°1
1875.....	131	1°51	—0°2
1885.....	127	1°60	—0°4
1903.....	117	1°51	+0°5

Gore in 1887 computed an orbit which gave a period of 486 years.

Auwers gives the proper motion of AB as

— $0''083$ in R.A. and — $0''008$ in N.P.D.,

which must be shared in by the third star C, since the measures indicate a very slight motion only. Now the distance of C is $8''6$, and if we suppose the mass insignificant with respect to the combined mass of A and B, the period would be some 8600 years, or an annual motion in angle of $0^{\circ}04$, a quantity which well represents the measures. The magnitude of C is 7.4; its colour is blue.

Σ 2021. R.A. $16^h 8^m.6$, N.P.D. $76^{\circ} 12'$.

There are many measures of 49 Serpentis, another of Sir W. Herschel's discoveries, and the motion at first sight appears rectilinear. If, however, the motion be analysed, we obtain annually

from 1783-1803	a motion of $0''033$
1803-1835	„ $0''025$
1835-1865	„ $0''022$
1865-1898	„ $0''017$

indicating orbital motion. The character of the pair is placed beyond doubt when we consider that it has a large proper motion of

— $0''157$ in R.A. and + $0''416$ in N.P.D.,

evidently common to the two components. The period would be about 500 years.

1903 Angle 335° . Distance $3''8$.

The magnitudes are 6.7 and 6.9.

T. LEWIS.

[To be continued.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Transits of Mercury in 1815 and 1835.

GENTLEMEN,—

The recently published 'Nautical Almanac' for 1907 contains, as a matter of course, elements of transit of δ on November 7 of that year. Seventy-two years before that date, or on November 7, 1835, there happened one of the longest of these transits during the last century. Newcomb, in 'Transits of Mercury,' vol. i. part vi. (Washington, 1882), remarks :—" Among the unobserved transits it is most surprising that that of 1835 appears to have passed unnoticed, although the ingress was visible all over the United States, it having occurred about Washington noon." The 'American Almanac' for 1835 devotes three closely printed pages to the phenomenon, and gives calculated times of contacts and other particulars for forty-seven different places in America. But the subsequent issues for 1836 and 1837 do not contain a word concerning the observation of it. The transit of November 7, 1835, would also be favourable for observation in Australia. Yet, though the transit of 1822 was observed by several in that land, there does not seem to be a single account of the latter one.

On November 12, 1815, Mercury would pass across the Sun for the second time in the last century, and on this occasion India, as well as Australia, was very favourably placed for the observation. Frend's 'Evening Amusements' for that year has the following :—" It would not be beneath the dignity of the East India Company to give notice of this event. . . . Benares was famous, formerly, for its observatory, and India possesses Europeans of distinguished mathematical talents, on whom, we may be persuaded, this phenomenon will not be lost. The governments of Europe patronised the travels of astronomers to observe the transits of Venus, and it will be an honour to the East India Company if it should be the first to produce to the world an accurate account of the transit of Mercury." On applying to the India Office lately on this matter, I was informed that nothing could be traced as to any record of an observation. Astronomy, with the exception of the observations carried on at Madras, appears to have been at a very low ebb in India about that time. Markham (in 'Memoir of Indian Surveys'), in the course of a chapter on astronomical work there from the earliest times, mentions the establishment of five observatories by Jai Sing at Delhi, Jaipur, Mattia, Benares, and Adjain respectively, but these appear to have fallen into decay, for Markham writes : " This great and wise prince Jai Sing . . . died in 1748, . . . and Hindoo science expired with him." Later, in the same chapter, Markham writes : " The Madras Observatory is now the sole

permanent point for astronomical work in India, and the only successor of the famous establishments founded by Jai Sing." As there is no account of the transit in the Madras Observatory series, we must conclude that no scientific record of this remains.

It certainly seems strange beyond measure that two transits of Mercury, occurring less than a hundred years ago, should not have been observed. It is just possible that some sort of observation by a private individual might be brought to light, just as an account of the total eclipse of the Sun in 1832 (in some respects the most remarkable of all those in the 19th century) was furnished me a few years ago by an eye-witness who saw it when she was a girl 12 years of age.

Faithfully yours,

Melplash Vicarage, Bridport,
1904, April 28.

S. J. JOHNSON.

P.S.—I may here mention that I noticed the planet yesterday evening for the 150th time with the naked eye after sunset.

Errors of Hansen's Lunar Tables.

GENTLEMEN,—

It may be worth while to call attention to a small inadvertence in the tables of comparison between the places of the Moon deduced from Hansen's Tables and the results of the Greenwich Observations as given in the Appendix of vol. I. of the 'Monthly Notices of the R. A. S.' The comparison had already been made in the Greenwich Observations for 1859, but it was thought desirable to make it more strictly accurate as regards the exact time at which the observations (with which the tables are compared) were made. The inadvertence to which I refer occurs in a portion of the headings. In the above volume of the Greenwich Observations, where the first comparison was made (for the details of which, after the places had been computed, I am myself responsible), the headings of the portion referring to the observations made with meridian instruments are given as "Meridional Observations," so as to include in one term those (from 1847 to 1850) made with the transit-instrument and mural circle, and those (from 1851 to 1861) made with the transit-circle. But in the later comparison, printed in vol. I. of the 'Monthly Notices,' all are said to be "from observations made with the transit-circle."

Yours faithfully,

Blackheath, 1904, April 9.

W. T. LYNN.

The Constellations Triangula.

GENTLEMEN,—

There are three triangles in the heavens. Ptolemy's list includes the first, which is situated between the southern parts of Andromeda and Perseus, and has two stars (α and β) between the third and fourth magnitudes, and two others (γ and δ) somewhat

fainter, all which are catalogued by Ptolemy. Hevelius catalogued five others, making nine in the constellation; but he calls it *Triangulum majus*, and forms another, joining it on the south, which he calls *Triangulum minus*, and catalogues in this three stars, all of about the sixth magnitude. Flamsteed, however, ignores the division (which is recognized in Bode's Atlas), and calls the whole simply *Triangulum*. In the B. A. C., also, these stars are denominated, according to their numbers in the '*Catalogus Britannicus*,' 10, 11, and 12 *Trianguli*. γ and δ , it may be remarked (8 and 9 in Flamsteed's list), are very close together, and are of about the fifth magnitude.

The last of Bayer's maps contains figures of constellations near the south pole from the reports of navigators, without attempting to indicate the individual stars in them. One of these is *Triangulum australe*, which was afterwards lettered by Lacaille. It follows *Lupus*, further to the south, and has one star (α) of nearly the second magnitude, and two others (β and γ) of nearly the third. α and γ are both less than 22° from the south pole.

Yours faithfully,

W. T. LYNN.

Blackheath, 1904, April 18.

OBSERVATORIES.

OXFORD UNIVERSITY.—The main fact of Prof. Turner's Report for the year ending 1904, April 30, is that the Oxford Section of the Astrographic Catalogue is finished, so far as measures and reductions are concerned, and it is necessary now that the results should be printed. But here lies a difficulty—the University Press were unable to print the Catalogue without an adequate subsidy. A Committee appointed to consider the matter recommended the Hebdomadal Council to inform the Visitors to the Observatory that, in the opinion of that Council, speedy publication was desirable, but that the University Chest could not find funds. With this opinion in support, application was made to the Royal Society, who approached the Government with a view to the provision of the necessary £2000. The result of the application is not yet known.

As the Astrographic work is finished, it is necessary to find new fields of activity. There was a suggestion that another zone might be partially undertaken, and experimental measures were made of some plates taken at the Perth Observatory, West Australia, but no funds are yet forthcoming to carry on this work. A small instalment has, however, been received from the Government Grant Fund for the measurement of the Eros plates, which will soon be undertaken.

Reference is made in the Report to the magnificent gift of the "Stereo-Comparator" which the Observatory has received during

the year from Mr. C. L. Brook, F.R.A.S. To the editing by Prof. Turner of the Rousdon Variable-Star Observations, and to the discovery of Nova Geminorum. The following paragraph referring to the Nova is of interest :—

But the special problem of interest solved during the year was this :—two photographs taken independently by Dr. Max Wolf at Heidelberg, and by Mr. Parkhurst at the Yerkes Observatory, before the outburst, indicated a faint star very near the place of the Nova, with which it might be identical : was it in fact identical or not ? Copies of these photographs were kindly sent to Oxford and measured with the greatest care ; and the evidence was against identity. The small star seemed to be at least five seconds of arc away from the place subsequently occupied by the Nova. While the Nova was bright it was impossible to detect a faint star so near it as this ; but the light of a new star gradually wanes, and by waiting long enough it should become possible to detect this small companion, if the photographs had been rightly interpreted. This crucial observation was first made by the sharp eyes of Professor Barnard, using the giant refractor of the Yerkes Observatory. He wrote to me under date September 1 :—“This morning I got your Nova in Gemini. There is a faint star, 14 mag., preceding it, which I measured, and which seems to be Mr. Parkhurst's star.”

Mention is again made of the need of a dwelling-house attached to the Observatory.

TACUBAYA, MEXICO.—Our last notice* of this Observatory, which was kindly supplied to us by the Director himself, M. Felipe Valle, who has held that position since 1899, contained a succinct account of the work from the time of his appointment until the end of 1901. We have lately received a printed report covering the period from the latter date to the end of 1903 June. The Director reports to the Government at the end of each three months the details of the work done during that period, and makes a summary for the fiscal year, which it seems ends in June. This summary for last June stated that with the meridian circle 2258 observations of fundamental stars had been made with the meridian circle, and 2353 of stars to be used as reference-stars for the astrographic work taken up by this observatory. 274 catalogue-plates were taken during the year, of which 239 were considered satisfactory ; 95 catalogue-plates were measured, containing 37,482 stars, and 7 plates of Eros fields, which measures, it is believed, will be handed to Mr. Hinks for incorporation in his parallax determinations. Besides this, physical observations of the Sun are made regularly, and meteorological observations.

The Report closes with three specimens of the work—some pictures of the lunar eclipse of 1903 October 16, a table of observations of Comets (*d*) 1902 and (*a*) 1903 and some small planets, and the meteorological records for April, May, and June, 1903. Thus the National Observatory of Mexico appears to be fulfilling its purpose satisfactorily.

* 1902 April.

HARVARD.—There is a great similarity about Prof. E. C. Pickering's Reports; that for the year ending 1903 September 30 begins with a request for money, about 50,000 dollars, for building and other purposes connected with the Observatory. The work done during the year was in the main photometrical, in continuation of various researches detailed in former reports. Prof. Wendell has continued to use the polarizing photometer on the East Equatorial; it is stated that "the errors of observation with this instrument for a set of sixteen settings have an average value of from three to four hundredths of a magnitude. Deviations of a tenth of a magnitude generally indicate real changes in the star measured." The Director has continued to use the meridian photometer. The meridian observing has been confined to determination of clock-error. The reduction of observations and compilation of the *Ast. Gesell. Catalogue* is proceeding. Photographs of spectra of stars continue to be taken with the 11-inch Draper telescope. Large-scale photographs with the Bruce photographic telescope, and on 18 plates of this series Mr. Frost found 37 unrecorded nebulae, and on one plate he counted 10,926 stars in one square degree. The Boyden Department still remains at Arequipa under the charge of Prof. Bailey.

KODAIKANAL AND MADRAS.—Mr. Michie-Smith, the Director of the Kodaikanal Observatory, came to England on leave in 1902 October, and Mr. C. P. Butler, A.R.C.S., was appointed Acting Director. The Report of the Observatory for 1903 is signed by Mr. Butler. A programme of work in solar physics has been drawn up by a committee, but when the Acting Director took charge he found the three existing spectroscopes inadequate, so he combined parts of two of the instruments, and made a fairly-efficient spectroscope, with which sun-spot spectra have been observed on every available occasion. The result of this work is stated as follows:—"It is evident that the spectrum of the spots has been identical in every respect with that seen during the last four or five years, the twelve most widened lines being the same as those recorded visually at South Kensington and photographically at Yerkes."

With the same instrument the chromosphere is examined daily when possible, and the positions of the prominences measured. The 4-inch Dallmeyer photoheliograph has been overhauled, and solar pictures 8 inches in diameter were taken on 70 days between August 22 and December 31. A spectroheliograph for photographing the Sun in monochromatic light on Prof. Hale's plan has been ordered from the Cambridge Scientific Instrument Co. Meteorology, Actinometry, Seismology form part of the programme, and both changes and absolute values of the magnetic elements are observed and recorded, for Kodaikanal is one of the base stations of the Magnetic Survey of India now proceeding.

The Madras Observatory, which is in the charge of Mr. R. Ll. Jones, Deputy Director, is devoted mainly to Meteorology and Time Service; the only change made during the year is that a time-signal is now sent to Colombo.

STONYHURST.—In the year 1903 the solar surface was observed on 207 days notwithstanding the unfavourable weather, and 194 drawings were added to the series, with notes of clear surface on 13 dates. Cloudy nights prevented much progress in stellar work, and only 141 photographic spectra of stars were added to the collection. The large grating solar-spectrometer has been practically out of use during the year. It seems that to satisfactorily observe the spectra of spots it is necessary to have a larger image of the disk than is at present possible.

The astronomical instruments of the late Col. Cross's Observatory at Redscar have been presented to the Stonyhurst Observatory by his son. The smaller of the two equatorial polar axes, built for a 7-inch Newtonian reflector has been mounted in a revolving shed, and is intended to carry the 4-inch prismatic camera, as soon as some difficulties connected with the clock-driving have been overcome. The two spectrographs will then be in operation together, one on the Perry equatorial for the blue and yellow regions, and the prismatic camera for the violet and ultra-violet spectrum.

PUBLICATIONS.

SOME VARIABLE-STAR OBSERVATIONS *.—In these days of large telescopes, photography, and spectrography, it is apt to be forgotten that there have been astronomers who have made reputations and done good work with much less adequate instruments, and, in fact, with no instrument at all. Among these was Edward Heis, Director of the Observatory of Münster, in Westphalia, from 1852–1877, and Professor of Astronomy and Mathematics in the Academy of that city.

Heis will probably be remembered by his 'Atlas Cœlestis Novus,' a series of twelve maps showing nearly all the stars seen by the naked eye from Münster, the number of which in Heis's case was excessive, owing to his acute vision, with their relative magnitude, the result of 27 years' work. At the same time that he was making the Atlas, he also made observations of the change of magnitude of the variable stars visible—at least at maximum—to the naked eye. At the present time there are 39 such stars

* 'Beobachtungen veränderlicher Sterne von Eduard Heis 1840–1877 und von Adalbert Krueger 1853–1892.' Berlin: Felix L. Dames, 1903. Price 2os.

known in the northern hemisphere, and 16 of these were observed by Heis. His original observations of these have lately been edited by Father Hagen, S.J., of Georgetown College, Washington, in conjunction with Rev. E. Goetz, S.J., now of the Buluwayo Observatory, and Rev. R. Martin, S.J. The same book contains also, and by the same editors, the observations of variable stars and of several Novæ made by Dr. Krueger in the years 1853—1892.

Heis made his observations by naked eye, with an opera-glass, a comet-seeker, or a small telescope not equatorially mounted. His method was that known as "steps," in which certain comparison-stars are chosen, separated in magnitude by certain "steps," which have their equivalent in magnitude, and the observation consists in noting by how many steps one or each of the comparison-stars differs from the variable. Series of such observations made by Heis of the stars α Cassiopeiæ, Mira Ceti, Algol, λ Tauri, ϵ Aurigæ, α Orionis, ζ Geminorum, R Hydrae, α Herculis, R Scuti, β Lyræ, χ Cygni, η Aquilæ, μ Cephei, δ Cephei, β Pegasi are given in this book, each observation being reduced to show the equivalent magnitude in steps, so that there are records of the changes of magnitude of these stars, each extending over 20 or 30 years. Krueger's observations are of the same kind, and in some cases are of the same stars as in Heis's list, but the series are not so long. Krueger's observations also comprise estimations of the magnitudes of Nova Coronæ, Nova Andromedæ, Nova Aurigæ, and the pseudo-variable δ Orionis.

CALCULATING TABLES*.—We have received for review a copy of Dr. Zimmermann's 'Calculating Tables,' which we have no hesitation in recommending as a convenient aid to computation. The book measures 10 by 6 inches, and is not a quarter of an inch thick, so that the excessive cumbrousness of some books of this class does not exist in this case. The principal feature of the work is a huge multiplication table, multiplying three digits by two digits up to 1000 times 100, with notes at the foot of each page of products giving squares, cubes, roots, and logarithms, with other useful tables and constants in other pages, such, for example, as a table of factors of all the odd numbers up to 999, and of course, incidentally, the primes.

The book has evidently been prepared with great care, so much so that in the Preface, which is dated March 1889, it is stated that the publishers would give ten shillings to anyone who detected a mistake. It would have been interesting if they had told us in this edition how many half-sovereigns they have had to pay.

* 'Calculating Tables.' By Dr. H. Zimmermann. Translated from German into English by L. Descroix. Asher & Co., London, 1904. Price 6s. net.

THE GEOGRAPHICAL WHO'S WHO*.—The publishers have kindly sent us a useful book whose exact title 'Geographen-Kalender' is thus familiarly translated. One item of its contents that specially recommends itself to us is a list of the scientific institutions of the world arranged under heading of country, and then divided by subheadings, indicating the sciences they represent, *e. g.* the observatories of France, America, and other nationalities are all tabulated under one subheading. There must necessarily be some mistakes in a work of this kind, and we find Col. Tupman's observatory given in the American section; but some small errors do not much lessen the usefulness of the arrangement. There is a great deal more information of a kindred nature to this, an almanac, and tables of geographical data in measures of different nations. The frontispiece of the book, which is of German origin, is a portrait of Sir Clement Markham.

NOTES.

COMET NOTES.—There seems to be no reason for supposing that the orbit of Brooks' Comet (*a* 1904) differs greatly from a parabola.

E. Strömgren gives the following elements in *Ast. Nach.* 3947, from observations extending from April 17 to 26; it will be seen that they are practically identical with Ebert's elements given last month (the latter were erroneously printed; the symbols ω and Ω should be interchanged):—

T		1904 Feb. 28·813	Berlin M.T.
ω		50° 51' 30"	} 1904·0.
Ω		275 17 36	
<i>i</i>		124 59 38	
log <i>q</i>		0·42951	
<i>q</i>		2·688	

Ephemeris for Berlin Midnight.

	R.A.			N. Dec.		R.A.			N. Dec.
	h	m	s			h	m	s	
June 3....	13	45	51	57 48	June 19....	13	0	10	55 29
7....	13	32	15	57 22	23....	12	52	14	54 44
11....	13	20	8	56 49	27....	12	45	36	53 58
15....	13	9	27	56 11	July 1....	12	40	11	53 12

Brightness 0·64 on June 3, 0·42 on July 1, that at discovery being unity.

On May 19 Strömgren's R.A. was 41° too great, his Declination 2·2 too small, showing that his elements are not greatly in error.

* 'Geographen-Kalender,' Zweiter Jahrgang 1904/05. Justus Perthes, Gotha. Price 4 marks.

Hence it appears certain that the supposed places of the comet on March 11 and 15, as published from Harvard photographs, do not belong to this comet, but to some other object or objects, for they are some 6° distant from the positions deduced from Strömberg's elements.

The large perihelion distance of this comet is noteworthy. In this respect it resembles the comet discovered by M. Giacobini a year and a half ago.

A. C. D. C.

MINOR PLANET NOTES. — The following new planets are announced:—

Planet.	Date.	Discoverer.	Place.	Mag.
NZ	1904 April 19	Dugan.	Heidelberg.	13·2
OA	19	"	"	13·0
OB	21	Wolf.	"	13·2
OC	May 7	Dugan.	"	12·2
OD	11	"	"	12·2

Planet 498 has been named Tokio, as this planet proved to be identical with FF, discovered by Hirayama in Japan in 1900. The designation seems particularly appropriate at the present time.

Permanent numbers have been assigned to recently discovered planets as follows:—LY 513, MB 514, ME 515, MG 516, MH 517, MO 518, MP 519, MV 520, NB 521.

The planets LZ, MC, MD, ME, MK, MM, MN, MQ, MR, MS, MT, MU, MW, MX, MY do not receive permanent numbers, not having been sufficiently observed; MI was cancelled as erroneous; MA, ML, MZ, NA are identical with 184 Deiopeia, 214 Aschera, 409 Aspasia, and 505 respectively.

The *Berliner Jahrbuch* for 1906 contains new elements of the interesting planet 475 Ocllo, by Prof. Kreutz.

The eccentricity of its orbit is 0·380, the greatest of the entire family. The least, mean, and greatest distances from the Sun are 1·60, 2·59, 3·58 respectively. The period is 4·162 years, so that six periods are almost exactly 25 years, and the most favourable oppositions are separated by this interval.

The *Berl. Jahrb.* contains the usual statistics about the minor planets: 361 of them have been observed at four or more oppositions, 29 at three, 43 at two, and 85 of them (not including the recent discoveries) at only one opposition. Of the planets numbered 1 to 200, five are missing, viz., 99 Dike, 132 Aethra, 1 Scylla, 157 Deianira, 193 Ambrosia.

Prof. Kurt Laves contributes a discussion of the orbit of 34 Chicago to the 'Decennial Publications of the University of Chicago.'

This is one of the interesting planets whose periods are nearly commensurable with that of Jupiter. It forms one of the so-called "Hilda" group, whose mean daily motion is about $450''$, that of Jupiter being $300''$. The other three planets of the group (viz., Nos. 153, 190, 361) have notable librations produced by Jupiter, but these are shown to be less marked in the case of 334. The author and Prof. Moulton are constructing tables of this planet, and give the first part of the computations. Incidentally they call attention to the desirability of rediscovering 132 Aethra, since its accepted period is almost exactly one-third that of Jupiter.

A. C. D. C.

SUN-SPOT VARIATION IN LATITUDE.—Dr. W. J. S. Lockyer has lately communicated to the Royal Society a paper on this subject, but the point he makes is not entirely novel. The well-known law of Spörer states that a little before minimum spots appear far from the solar equator. After minimum they leave high latitudes, and the *mean latitude* of the spots seen on the Sun constantly diminishes with the next minimum. This law is quite borne out by observations, Dr. Lockyer agrees; but he says it does not state all the facts, and he draws curves to show that after a mass of spots breaks out in high latitudes, the mean latitude gradually gets smaller, but after a year or two there is another outburst in high latitude, and there is again a cycle of spots of gradually diminishing latitude, so that there are two or more of Spörer's cycles going on at the same time, and that to study the latitude of sun-spots properly it is not sufficient to take the mean of all the spots in each of the hemispheres, but that the solar surface should be divided into zones, each three degrees of latitude wide, and the mean latitude and the number and area of spots seen in each tabulated, year by year or rotation by rotation.

The fact of these double and triple cycles seems to result from the diagram published in the 'Mon. Not. R. A. S.' for June 1903, tabulating the Greenwich records of sun-spots. Anyone looking at the diagram can see apparent second and third outbursts in high latitude after the original one at minimum, but it is doubtful whether there is evidence for any other laws so well founded as Spörer's.

THE GREENWICH VISITATION DINNER.—Unlike those of previous years, the card of invitation to the Greenwich Visitation this year bears no mention of a dinner to be held afterwards, when the Board of Visitors would meet the Astronomer Royal and his staff. There is some doubt about the exact date when the Members of the Board first met to dine, but there is record of a Visitation Dinner on 1817, July 12, at 4 o'clock, to which Mr. Pond issued invitations. The next record of the dinner, which may or may

not have been intermitted, is for the year 1837, and contains the entry: "9 watermen's dinners with beer and grog 22s. 6d." In those years the dinner was held at the "Crown and Sceptre," one of the Greenwich taverns, which at that time had great reputations, but in later years (about 1863) it became established at the "Ship." In the "eighties" the charm of a Greenwich fish dinner was on the wane, or possibly diners became more critical, but at all events the feeling arose that more satisfaction could be obtained elsewhere, and after a spasmodic attempt at improvement in 1888, when trial was made of another Greenwich hostelry, the dinner was held at the Criterion Restaurant, Piccadilly Circus, in 1891, and all years subsequent until last year. Up to about 1894, 40 or 50 persons, or possibly more, sat down, but the number at the table has gradually decreased, so that it has been thought advisable not to hold the dinner this year. Incidents of past dinners are difficult to collect, as of course no record is kept; but one who has been present on many of the occasions can recollect having seen many famous men at the board—Sir J. Herschel, Sabine, De Morgan, Whewell, Babbage, Sidgwick, Adams, Huxley, Lord Kelvin. Huxley is especially remembered as a brilliant and humorous chairman, whilst Prof. Pritchard generally contributed his share to the gaiety of the feast. For the most part the character of the dinner was staid and decorous. There was a time when smoking was not allowed at the table, but one can remember a certain noble lord, who was sitting as Chairman by reason of his position as President of the Royal Society, calling upon his right-hand neighbour for a song, and, the call being responded to, the proceedings became of a musical, not to say "free and easy" nature. But there is to be no dinner this year, and so—perhaps temporarily—an old-established custom passes away.

THE VARIATION OF LATITUDE.—Near by year Prof. Albrecht publishes the results derived from the observations at the six stations established for the purpose of determining the variation of latitude. In *Ast. Nach.* No. 945 he gives the results for the year 1903, with a diagram showing the motion of the Earth's north pole derived therefrom. As to this it may be said that the diagram is much as it was expected to be. The pole described an almost complete circular course of about 0".20 radius, similar to that of the years 1897, 1898, and in 1890, 1891. See diagrams in this magazine for 1895, March, and 1900, September.

AN IMPROVED DIPLEIDOSCOPE.—This instrument may be defined by saying that it lies between the sun-dial and the transit-instrument. Its main principle is a right-angled prism properly adjusted, in which can be seen two images of the Sun when near the meridian—one seen by reflection from the first surface of the prism, and

another, very brilliant, being doubly reflected from the two inner surfaces. These images approached one another and coincide when the Sun is on the meridian.

The difficulties of the observation arise from the comparatively excessive brightness of the reflected image and from the fact that it is not easy to note the exact coincidence.

Sir Howard Grubb has overcome these difficulties by covering one half of the prism with a film of sulphide of lead so as to make the two images equal in intensity, and by adding a lens so that the images are thrown on to a screen. With an instrument recently exhibited, the solar images 2 inches in diameter moved relatively to one another at the rate of about one-thirtieth of an inch per second, so that it is quite possible to easily register time within one second.

THE ROYAL SOCIETY CONVERSAZIONE.—Astronomy was represented at this function by Mr. Franklin-Adams' photographs of the Southern Heavens; by photographs and diagrams illustrating Solar and Meteorological changes and the determination of the relative temperatures of stars, exhibited by Sir Norman Lockyer. Transparencies of negatives taken with the Yerkes spectroheliograph by Prof. Hale and Mr. Ellerman were exhibited by the Royal Astronomical Society.

THE following circular letter has been received from Prof. Cleveland Abbe, of the Washington Weather Bureau :—" You may have noticed that records have been published showing that an appreciable general diminution of the transparency of the Earth's atmosphere took place some time during the year 1902, but disappeared at some time during 1903. This is an important matter, and may possibly be made the basis of an explanation of other meteorological phenomena. I beg to ask whether you have any records that will assist in fixing the dates of beginning and ending, and the extent of this change in transparency. Such records may consist of photometric or photographic observations of the brightness of the stars; changes in the solar or stellar spectra; unusual prevalence of halos, large Bishop's ring, or haze; observations of heat received from the Sun, as made with actinometers or pyrheliometers; observations of the polarization of the blue sky light and of scintillation of the stars.

"Undoubtedly this diminution and increase of transparency began and ended at different dates at different places, as the phenomena spread gradually over the world during the years 1902 and 1903; additional records are therefore desired in order to trace its progress. Will you not kindly examine your records from this point of view, and send me the result for publication in a general article on this subject?"

A SMALL observatory has been opened at the Passmore-Edwards Sailors' Palace, Limehouse, for the use of the students of the King Edward VII. Nautical School, located at the institution. The equipment consists mainly of an 8-inch equatorial by Sir Howard Grubb, housed in a steel observatory, all of which was the gift of the late Lady Louise Ashburton, mother-in-law of the Marquess of Northampton, who opened the observatory on May 13.

PROF. G. H. DARWIN has been nominated President of the British Association for the Meeting in 1905, which will be held in South Africa.

M. G. BIGOURDAN, Astronome-Adjoint of the Paris Observatory, to whom we offer our congratulations, has been elected a Member of the Academy of Sciences in the Astronomical Section, in place of M. Callandreau, who died last year.

PROF. G. H. DARWIN has been elected a Foreign Associate of the U.S. National Academy of Sciences.

THE next Meeting of the Royal Astronomical Society will be on Friday, June 10; of the British Astronomical Association on Wednesday, June 29.

FROM AN OXFORD NOTE-BOOK.

WE had a great scene in Oxford on Tuesday, May 17. There has been a question of reform in the examinations in Theology. Hitherto the examiners have been necessarily clergymen of the Church of England, though men of all religious opinions are examined; and it was thought the time had come when this limitation might be removed. So, at any rate, thought the body of residents in Oxford before whom, in "Congregation," it is necessary to bring all important reforms in the first instance. But to confirm them as law the further approval is necessary of the much larger body of non-resident graduates who have kept their names on the books of the University, and the curious nature of this larger Oxford was sensationally exemplified on May 17. Coming to the Sheldonian Theatre (where, in expectation of a large assembly, the discussion and vote were to be taken) a few minutes after 2 o'clock, I found it scarcely possible to get inside. The floor was absolutely black with country clergymen, packed almost as close as they could stand. They had rushed up to Oxford in the belief that the Church of England itself was threatened, and made it very clear that they would listen to no persuasion to the contrary. When a mild supporter of the reform tried to explain it to them, they howled. So at last a vote was taken, and by 676 to 278 the base conspiracy was defeated and

the victors returned triumphantly to their country parishes. But there was something rather saddening in this sudden glimpse of the populace of a great University.

TURNING over the leaves of Newman's 'Apologia' the other day, to see something of what Oxford was like, theologically, half a century ago, I was interested to find that his last point of contact with Oxford was the Radcliffe Observatory. Here is the passage :—

I left Oxford for good on Monday, February 23, 1846. On the Saturday and Sunday before, I was in my house at Littlemore simply by myself, as I had been for the first day or two when I had originally taken possession of it. I slept on Sunday night at my dear friend's, Mr. Johnson's, at the Observatory. Various friends came to see the last of me: Mr. Copeland, Mr. Church, Mr. Buckle, Mr. Pattison, and Mr. Lewis. Dr. Pusey too came up to take leave of me; and I called on Dr. Ogle, one of my very oldest friends, for he was my private Tutor, when I was an Undergraduate. In him I took leave of my first College, Trinity, which was so dear to me, and which held on its foundation so many who had been kind to me both when I was a boy, and all through my Oxford life. Trinity had never been unkind to me. There used to be much snap-dragon growing on the walls opposite my freshman's rooms there, and I had for years taken it as the emblem of my own perpetual residence even unto death in my University.

On the morning of the 23rd I left the Observatory. I have never seen Oxford since, excepting its spires, as they are seen from the railway*.

* At length I revisited Oxford on February 26th, 1878, after an absence of 32 years.

ASTRONOMERS cannot fail to take an interest in the growth of the National Physical Laboratory, if only for the reason that the nation is more or less following in the case of physics the precedent set in the establishment of a national astronomical laboratory. We call the latter by a different name, but there is much to be said for adopting the suggestion made by Professor Schuster (in his British Association Address to Section A in 1902) and re-christening our observatories as laboratories: for to observe phenomena is after all not the important part of the scheme—the observations are worthless unless we work at them. At any rate the differences between observatories and laboratories are becoming more and more ill-defined in these days of comparison-spectra and measuring-machines. An additional point of interest is that the astronomical precedent has been followed at the respectable interval of something over two centuries. What, I wonder, will be the new laboratory of two centuries hence?

The report of the Executive Committee for 1903 shows rapid development of the work, on which they are to be cordially congratulated; and anxiety about funds, in which many of us will be able to sympathize with them from experience. The number of tests

made in nine months of 1902 was 269; and in 1903 this number increased to 1330. The staff, too, has increased from 26 persons to 34. But money is badly wanted both for the equipment, for the actual work, and for better salaries, only one salary exceeding £200 a year. It is to be sincerely hoped that the budding institution may be speedily put on a proper financial basis. Two points of special interest to astronomers and meteorologists are:—first, that the magnetic observations are to be transferred from Kew to Eskdale Muir in Dumfriesshire, where “the Committee trust that the site may remain free from magnetic disturbance for many years to come”; and second, that there seems to be a possibility of inferring from experiments on models what the wind-pressure will be on structures of other dimensions, and a wind-tower is to be built to conduct the experiments.

THE excellence of the Repsold registering-micrometer for transit observations has already been universally recognized, and there is no novelty in singing its praises. But each new proof of its value is worthy of remark, and the recently published determination of the longitude Potsdam—Greenwich presents such evidence very clearly and succinctly on pages 76 and 77. It gives one quite a new sensation to see a column headed “Personal Equation” filled with a uniform row of zeros in 3 decimal places, 0^o.000; and to see the definitive result for personal equation stated as

$$0^{\circ}.000 \pm 0^{\circ}.005.$$

Even the beautiful residuals for the separate nights, rarely rising to 0^o.02, which are very striking, take a place second in interest to this wonderful elimination of personal equation, which has not been bettered, if indeed it has been equalled, by any photographic method.

ACCORDING to some other accounts, this beautiful instrument does not entirely obliterate, though it much reduces, personal equation. We must await further information before we can decide completely. But it would be quite in accordance with expectation if personal equation is found to be practically annulled. It was stated some years ago * in these notes, and it has probably been maintained by others long before that time, that if we could employ *one* sense only, instead of *two*, in observing, personal equation would disappear. This troublesome systematic error arises from the imperfect co-ordination of two senses, whether seeing and hearing, or seeing and touch. The new micrometer needs the eye only.

* See *Observatory* for August 1897, p. 331.

It has been decided by the Council of the Royal Astronomical Society that henceforward each separate paper which appears in their 'Memoirs' shall be circulated at once, without waiting for others to form a volume. If we are not mistaken, this will be welcomed as a great convenience to workers; and the little extra trouble needed in collecting several papers to form a volume will not be grudged. This change is in a direction in which several important steps have already been made elsewhere: notably in the United States.

MR. WILLIAM ELLIS, F.R.S., has been courageously attacking the false prophets. In Symons's *Meteorological Magazine* for April he contributes an article on "Some Weather Prophets," asking "how it happens that men, otherwise of considerable intelligence, can persevere in a task at present so utterly hopeless as the endeavour to predict detailed weather changes for long periods in advance?" Alas! the paradoxer is not likely to change his skin. He is, as he always was, often "otherwise of considerable intelligence," but none the less hopeless to argue with, and firmly convinced that men of science are banded into a conspiracy against him. One of those against whom Mr. Ellis has taken the field, in the next number hurls defiance and contempt at him and "scientific men, especially those connected with the Royal Society, the Greenwich Observatory, and the Royal Meteorological Society," who collectively show "great ignorance of the conditions of the weather problem as well as of the proper method of treating it." But he betrays the usual paradoxical anxiety for the good opinion of the men whom he professes to despise. After remarking somewhat incoherently that "Science admits no authority, no dogmatism, like that of Mr. Ellis and the Sun's heat," and that "A man like Mr. Ellis, who professes to believe that I predicted the weather correctly for 153 successive days by mere guessing, is capable of believing anything," he nevertheless goes on to put Mr. Ellis on to the judicial bench, or even above it.

Now I claim to have solved the problem of not only weather changes but also the incidence of great tides and earthquakes. In fact, I have found and proved by experiment that the atmosphere, the hydrosphere, and the lithosphere are tidally affected by the Moon and the Sun. And in order to establish this fact, I am prepared to predict the weather correctly by the tidal action of those great luminaries for any day or days in the future, if Mr. Ellis will be good enough to form a committee to adjudicate upon the result.—HUGH CLEMENTS.

I expect that, as the White King said to Alice, Mr. Ellis is *good* enough, but hardly *strong* enough, to form that committee. If he tried to catch anyone to form it, that alarmed person would "go by so fearfully quick" that "you might as well try to stop a Bandersnatch!"

THE above reference to "Mr. Ellis and the Sun's heat," though a little vague, is obviously intended to be crushing. But Mr. Ellis may be comforted by the thought that if, against his will, he is associated with such regions of high temperature, he will be at least in august company, for a Great Personage has been dealing with the Sun's heat :—

THE GERMAN EMPEROR.

(FROM OUR CORRESPONDENT.)

BERLIN, Friday Night.

The Emperor William has presented to the German Department of the Spirit Exhibition at Vienna a prize in the shape of a picture, under which is written, in his own hand, the following inscription :—

"The treasures in the depths of the Earth are exhaustible, but the Sun causes to mature inexhaustibly, in the potato, light, power, and warmth."

To anyone acquainted with the conditions prevailing in the agricultural districts of East Prussia and Prussian Poland, where the farm labourers receive spirit as part payment of their wages, the Emperor's allusion to the reproduction of the Sun's heat in the shape of potato spirit will be especially appreciated.

And so the idea of the gradual dissipation of energy has, at any rate so far as the Sun is concerned, received its death-blow !

It seems worthy of record that an astronomical analogy was used in the House of Commons on an important occasion. Mr. Black, Member for Banffshire, who on May 18 proposed an amendment which caused some sensation, is reported by the *Times* in one part of his speech as follows :—

The Prime Minister pursued the *role* of a political Nicodemus. While the right hon. gentleman was wobbling, the ex-Colonial Secretary was working— with the result that unless he was stopped the Government would come to play the part of a political comet, in which the right hon. member for West Birmingham would be the head, and the Government the large and nebulous tail. (Laughter and cheers.)

After allowing for the nebulosity, I suppose the main implication is that a comet travels head foremost, dragging the tail after it. I wonder whether, in that august assembly which laughed and cheered, there was a single individual who knew that just as often as not the tail is in front ?

THOSE who visit this year's Royal Academy Exhibition should take care to see the excellent bust of Halley (No. 1799), by H. Pegram, A.R.A. Judging from the several good portraits of Halley we have, the likeness is excellent, and the sculptor must have made the very best of his material. I believe the bust is to be presented by that well-known benefactor, Mr. Passmore Edwards, to the Free Library at Shoreditch, Halley's birthplace.

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EDITED BY

T. LEWIS, F.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

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VOL. XXVII.

JULY, 1904.

No. 346.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 June 10.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the last Meeting were read and confirmed.

Mr. Whittaker. 50 presents have been received since the last Meeting. Of those calling for special comment, I may mention that Prof. Pickering has sent the two plates of the Harvard Photographic Map of the Heavens, which were required to complete the 35 plates of the series; and from the Royal Observatory, Greenwich, we have received 21 photographic enlargements of plates of the Astrographic Chart (Declination $+65^{\circ}$, 66° , 67°).

The President. To these I may add a comparatively small but not insignificant present. I had never seen a portrait of Dr. Galle, our distinguished Associate who found the planet Neptune, and I asked him the other day if he would send me a photograph, which he has kindly done. This interesting picture will be passed round for inspection.

The President. Among those received for reading this evening are two important papers which both represent the result of some years' work. Mr. Hinks is able to give us the first proof of his investigation of the parallax of the Sun from observations of Eros, and Mr. Cookson has an interesting description of his work on the Satellites of Jupiter, done at the Cape of Good Hope. Then Mr. Gheury will give us an account of an instrument called a gyroscopic collimator, and Mr. Maunder and Father Cortie have each something to say about solar matters. I propose to take the papers in that order, and will therefore call upon Mr. Hinks to tell us something about his share in the campaign for determining the Solar Parallax.

Mr. Hinks. Two years ago I had the honour of presenting to the Society an account of some experimental reductions of a few photographs of Eros which had been reduced by a rectangular co-ordinates method which had some points about it. I then proposed to undertake a reduction of all photographs which might be available for the period Nov. 7-15, 1900, in order that we might have some idea of the pitfalls in the path of a man who undertakes a definitive combination of all the observations. At that time it did not seem that we had very much information as to the real accuracy of photographs when they were pushed to the limit, and we had very little knowledge indeed as to how the results of the Observatories would agree with one another. When I took the work up I hoped to be able to get the Solar Parallax with a probable error about as small as that derived by Sir David Gill, and to see whether the new direct observational method was going to stand by the old method or not. Two years ago I selected comparison stars, made a chart of them, and sent round copies to a number of Observatories with a request for measures. My request had a very gratifying response, and I cannot be too grateful to a number of Observatories that did me the honour of putting in my hands the results of observations made on those dates. (Screen.) When the reduction of these was taken in hand the first thing to do was to form a system of standard comparison stars, from measures made upon Cambridge and Paris plates, and it was estimated that the probable error would not be more than $0''.1$. I am glad to say that that estimate was outside the mark, because the probable error is actually about $0''.04$. Next we had to adopt a standard scale, to reduce all the measures to it, to correct them for the second order terms of the differential refraction, and then to transform them all to the same standard centre. Then they were all reduced to standard co-ordinates by the ordinary six constant method, and the residuals for the comparison stars were got out. Then the interesting part of the matter began; because as soon as I obtained those residuals it began to be evident there were curious large systematic errors in some of the results. The errors of the measuring-machine we may disregard; the errors of other *réseaux* I know little about, but I made an examination of my own and found that the photographed copy was sensibly accurate (except for gelatine distortion). The next thing one would naturally look for would be something in the category of hour-angle error, as Kapteyn calls it. One is always liable to this when a mirror is used; and there is always the chance of its arising from atmospheric dispersion. I satisfied myself that there was nothing in the Cambridge results which could be put down to hour-angle error. Then there came the question of guiding error and magnitude equation. I found indisputable evidence of guiding error in some of the Cambridge and Oxford plates, especially for one night at Cambridge, when I think something must have been wrong with the automatic control.

I now come to an error resembling an unsymmetrical distortion which I have found in the Lick plates and in the Algiers plates. The Lick plates were taken with the 3-foot mirror which used to belong to Dr. Common, and the Algiers plates were taken with a standard astrographic refractor. The error in the first may be attributed with certainty to optical distortion of images more than 12' from the centre, though why the effect should be unsymmetrical about the centre is a mystery. But it is decidedly disquieting to find that with the standard astrographic refractor one can get an error of this kind, the result of which is to displace the planet by half a second of arc. I mention these things to show that it is not all plain sailing when one begins to combine the results of different series of photographs.

We come now to the comparison with the ephemeris and the formation of the equations of condition. In speaking of the ephemeris I ought to express my gratitude to the Director of the Paris Observatory, who, although he disapproves of the method which I am using, took some trouble in furnishing me with the heliocentric ephemerides which I required. I should like to mention that specially, as I think it was very kind of him. I formed the equation of condition with three unknowns—the constant correction to the ephemeris, the correction depending upon the first power of the time, and correction to the assumed parallax. Although it did not seem at all unreasonable to expect that there was a term depending upon the square of the time, there might also be a small periodic term connected with the light-variation of the planet, and I decided to make a preliminary solution with the three unknowns. (Screen.) Here are the normal equations derived from 240 observations; the size of the cross terms will show how far bad weather has prevented a complete separation of the parallax from the other corrections. Having given the greater part of the observations full weight, and weighted down some sets that are clearly bad, I find the probable error of one equation of weight unity to be $\pm 0''.10$ in x and $\pm 0''.09$ in y . A discussion of the residuals leads to the conclusion that there is no sensible correction to the ephemeris involving the square of the time, but that there is apparently a real inequality in the x equations, whose period is half the $5^h 16^m$ which is given as the complete period of the light-variation of Eros. There is certainly no inequality with the full period, but I think that an inequality with semi-amplitude of about $0''.03$ in the half-period is probably real. This corresponds to a total shift of the light-centre of the planet of about 10 inches. This short-period inequality cannot affect our parallax equations sensibly. We may therefore adopt our preliminary solution as definitive, and find for our result $\pi = 8''.7966 \pm 0''.0047$. A part of my ambition is achieved when I find that this block of observations gives a probable error as small as that of Sir David Gill's, and a solar parallax agreeing

with his within the limits of the probable errors, and with the adopted value.

It seems to me that, having worked up this material so far, one can form certain conclusions, not only as to the carrying out of the final parts of the work, when the time comes, but also as to the plans to be adopted in many places where the work is scarcely yet begun. There were certain proposals made by the Eros Commission, involving an exceedingly elaborate system of measurement, of about 60 stars at least on each plate. Most people cannot afford to do that, and so it has come about that the French Observatories which are well provided with funds have taken the French official programme, but others have had to limit themselves to smaller schemes. For instance, the Crossley telescope took about 5000 exposures, of which 30 have been measured and the others are not yet begun; it cannot be possible to carry out the official programme in this case, and I think it is not necessary. It will be possible to select a system of comparison stars whose places can be determined from the French work; and everyone else might limit himself to measuring the planet and about ten of these selected stars. Only by adopting the reduced programme is there a chance of getting the work finished in a reasonable time. That is broadly the conclusion of this paper as to the future, and it would be useless to go further into detail now.

Sir Robert Ball. I should like to congratulate Mr. Hinks on this elaborate piece of work which he has carried out with such assiduity during these last two years. The results he has arrived at seem to be of very great interest as illustrating the power of this method, the nature of which it is hardly possible to offer any remarks upon until we have it in print, and have the opportunity of looking into the calculations more thoroughly; but I think we can congratulate Mr. Hinks upon having carried through with great zeal and enthusiasm a very valuable piece of work.

Mr. Hinks. I entirely omitted to make two acknowledgments: one is to my assistants Miss Bell and Miss Malden, who helped me with great skill and industry; the other is to the Government Grant Fund of the Royal Society, which generously gave us the means of securing their help, without which we could not possibly have done the work at Cambridge.

Mr. Dyson. I should like to endorse what Sir Robert Ball has said about the result that Mr. Hinks has secured from this heavy work. As one goes on with more photographs and tries to get results from them, one is bound to find a good many sources of error and difficulty such as Mr. Hinks has pointed out. Some of them we hope to get over, and some of them we shall not. The driving error is a case which I think Mr. Hinks puts very clearly, and the way in which he has got over that, and the way which I suppose others will adopt, is to take stars of the same magnitude as Eros for the comparison stars, using the brighter stars as reference stars for the position of the comparison stars. I may

mention a rather curious error which we noticed at Greenwich some time ago with regard to the photographs of Nova Persei. Nova Persei at the time was very bright, and we took photographs to find its parallax. One of the plates when Nova Persei was very bright showed a large residual of something under $0''.5$, and on examination we found the dome had not been quite clear of the object-glass. As regards the methods in which Mr. Hinks has treated some points, I do not quite agree with him, but one can hardly discuss our different views now. I should like to add my congratulations to those of Sir Robert Ball.

The President. I have listened to this paper with the very greatest interest, having seen something of the work, although not so much as I should like. I believe it is the first time that the photographic method has been tested in this way, that is, by bringing together plates from different observatories, possibly with different systematic errors, and comparing them so as to find their relative accuracy. Also we have now got some idea of the ultimate minute errors which affect the photographic method. Mr. Hinks has indicated, with great clearness and skill, some of them—I wish I could say all of them, and has shown that in spite of them the photographic method is capable of very great accuracy, and no better proof is wanted than that he is able to detect a minute variation in the rotation of this small body at a distance of so many million miles. The indication of an apparent change of position, although not quite certain, seems to point to the same period as that during which its light seems to vary, and showing that the illuminated area does not quite remain in the same place. If this should prove to be true, I think it will be almost as remarkable, if not more remarkable, than the observations of Sir David Gill when he determined the mass of the Moon with such accuracy. I congratulate Mr. Hinks and all those who have worked with him at Cambridge on the result of this first chapter in the work, because I think there will be other chapters to come. I will now ask Mr. Gheury to show us his improved form of the Gyroscopic Collimator of Admiral Fleuriais.

Mr. M. E. J. Gheury exhibited the latest pattern of Admiral Fleuriais's "Gyroscopic Collimator" (or Gyroscopic Horizon), and gave a description of the instrument, which is used in connection with a sextant for the purpose of obtaining an artificial horizon for observing altitudes at sea.

The principal feature of the instrument is a gyrostat revolving in vacuum and carrying an optical arrangement by which the image of a horizontal grating of a special kind is seen, by the light of an electric lamp or of the Sun (reflected), in the field of the observing telescope.

The motion of precession of the gyrostat gives to this grating a vertical balancing motion, and the gyrostat is therefore equivalent to a pendulum, the period of oscillation of which, being much

greater than the period of rolling of a vessel, is not disturbed by the latter motion.

The image of the observed celestial body is brought down on this field, as in the usual observation with the sea-horizon, and its position with regard to the lines of the grating is noted at three moments, where the grating reverses its motion. The average of these three positions is then taken as the position with regard to the sensible—or true—horizon, which is given by the centre line of the grating when the axis of the gyrostat is vertical; or a succession of positions are taken with the corresponding times, and from this a curve is plotted on which the position at any required time can be obtained. This gives a correction to apply to the reading on the limb of the sextant to get the true altitude of the observed body.

The instrument is set in motion in a couple of minutes with a hand vacuum-pump, two strokes of which are sufficient to produce a vacuum of 2 or 3 cm. of mercury in the gyrostat case. It is then ready for use.

The total weight of the sextant and gyrostat is $4\frac{1}{2}$ lbs., the weight of the gyrostat in its aluminium case being $1\frac{3}{4}$ lbs. This weight is not too large to be an objection to the use of the instrument, the observation lasting but a couple of minutes. For long series of observations a belt with supports for the right arm and for the sextant is used.

The accuracy is of the same order as that of the other kinds of artificial horizon for use at sea, that is, $1'$ to $2'$, while it can be used in weather that would prevent the use of another kind of apparatus, and is very handy. With a ship going at a high speed and rolling heavily, however, the errors may be $5'$ or $6'$, but these conditions are rarely co-existent with fog*.

Mr. Bryan Cookson then read a paper on "The Mass of Jupiter and Corrections to the Elements of the Orbits of the Satellites from Heliometer Observations made at the Cape in 1901 and 1902." The observations on which this investigation depends were made three years ago at the Cape of Good Hope with the large heliometer, the same as was used for observing the minor planets in the solar parallax campaign. The satellites were observed relatively to one another, the quantities measured being the distance and position-angle of each pair of satellites. This is the method of observing adopted by Sir David Gill in 1891 in his observations of the satellites, and is much more accurate than that employed by previous investigators, in which the satellite is referred to the limb of the planet. The present observations were made between June 24 and September 27, 1901, and between June 10 and October 5, 1902, that is, within two months of the opposition

* In answer to a question asked by the President, *Mr. Gheury* states that a sextant with the gyrostat and all necessary appliances can be bought for £26 from Messrs. Pouthusant Therrode, 6 Rue Victor Considérant, Paris.

of the planet. Altogether there were 783 observations of the satellites in distance and position-angle, each observation consisting of at least four separate pointings. To control two important instrumental constants, viz. the scale-value and the zero of the position-circle, a pair of standard stars was observed every night; the other instrumental adjustments were assumed not to vary, and were determined once for all by special observations. The meridian places of the standards are required with all possible accuracy, and for this purpose special observations of them have been made at several observatories. As the relative positions of the satellites are rapidly changing, it was necessary to have an assistant to note the instant of each pointing, and the corresponding Greenwich Mean Time was the argument with which the tabular places of the satellites were computed from an ephemeris based on the theory of Souillart. The differences between the observed and the tabular co-ordinates were then expressed in terms of corrections to the various elements of the orbits. All six elements of each orbit were considered as subject to correction, but the corrections to the major axes were expressed in terms of a correction to the mass of Jupiter and the compression constant Jb^2 ; corrections to the coefficients of the large inequalities of I., II., and III. were also introduced as unknowns. This gives altogether 25 unknowns, but in the equations of condition the coefficients of 11 or 12 of these unknowns were zero. After formation of the normals, preliminary solutions were made of the normals from each pair to see how the different determinations of the unknowns agreed with one another. The comparison is satisfactory, as the values of the mass show. The values of the mass from the general solutions of 1901 and 1902 are

$$1:1047.69 \pm 0.09 \quad \text{and} \quad 1047.66 \pm 0.06,$$

and though the agreement is good, still the difference between these and the accepted value, viz. $1:1047.35$, is so large that it requires explanation. Another important constant is the compression-constant of Jupiter, which may be determined from the observed secular motion of the node of the second satellite. A discussion of this shows that the adopted value is too great by about 11 per cent. It is well known that between the mean longitudes of the first three satellites there exists a peculiar relation, and Laplace proved by theory that there is a small oscillation or libration about the state represented by this relation. It has been possible from the present observations to determine the magnitude of this oscillation. The mass of Jupiter, his compression, and Laplace's libration are the three most important results of the paper.

Mr. Dyson. I think we shall all be sorry that Sir David Gill is not here to give us his remarks upon Mr. Cookson's paper. Not much use is made of the heliometer in this country, so that we are not able to discuss the observational details of Mr. Cookson's

work, but we know that 750 complete measures meant a good deal of observing. Possibly some Fellows have read the paper published by Dr. Hermann Struve with regard to a similar work on Saturn. That paper I read when it was published and I was struck with the number of points of interest which are involved in such a research as this. The difference between the mass of Jupiter found by Mr. Cookson and that found by Prof. Newcomb is one which requires some explanation. I take it the difficulty of getting masses from heliometer observations consists in getting the distances free from systematic error. I think Mr. Cookson is now reducing some observations from photographs, and they should be valuable as confirming or not his present results. I should like to ask how the deduced compression compares with visual micrometric observations.

Mr. W. W. Bryant. Has Mr. Cookson any new determination of the figure of the planet distinct from the compression?

Sir R. Ball. I should like to say what a beautiful piece of work this is of Mr. Cookson's. I should like to ask what is the value of Laplace's libration that he has derived from his investigation. It is an extremely interesting point.

Mr. Hinks. I should like to ask Mr. Cookson whether he has had to apply any systematic corrections, such as we have seen from time to time in parallax work?

Mr. Cookson. In reply to Mr. Dyson's question as to the observed distances, the average distance of a pair of satellites was about 450", the extremes were 3" and 900". The distance of the 1901 standard was 6500"; that of the 1902 standards was less, viz. 4500". There is a systematic correction to be applied to observations of small distances (*i. e.*, to distances between about 10" and 1000"), depending on the fact that you do not place the two images exactly one on top of the other, but side by side, in such a way that the line joining their centres is parallel to the sides of a carefully oriented square in the focus of the heliometer. This is a purely geometrical correction and the necessary readings were always taken, so that the proper correction could be applied to every observation. Beyond that no systematic correction has been applied. The residuals from the equations of condition were grouped according to the distance, but no dependence of the residual on the distance can be seen. In reply to Sir Robert Ball, the coefficient of the libration is $0^{\circ}.152$ or $547''$ —a surprisingly large value: but the utmost effect of it would be to retard or accelerate an eclipse by about 25 seconds, a quantity which might easily escape detection in the old eclipse observations, as they could only be observed to within about two minutes. The ellipticity of Jupiter's disc from direct measurement of his equatorial and polar diameters is about $1 : 16.5$, whilst the value corresponding to the value of Jb^2 found in the paper is $1 : 15.8$. The difference is considerable and may be due in part to the difficulty of measuring the planet's diameters: part may also be real.

The President. I scarcely know whether to hope that Mr. Cookson may find means of reconciling his results with those of Prof. Newcomb, or that he may find the difference well founded. When there is the possibility that the action of Jupiter on the bodies of his own system may be different from the action on other bodies passing near, the small discrepancy may lead us to some discovery about the solar system in the future. It has at any rate given us the opportunity of admiring the great accuracy with which these beautiful observations have been made, and I am sure you will return your thanks to Mr. Cookson for his paper.

Mr. Maunder then read a note on the distribution of sun-spots in heliographic latitude, 1874 to 1902. He said :—As the time is short, and the paper rather long, I think my simplest way will be to throw on the screen the diagrams with which it is illustrated. A little over a year ago, at the desire of the Astronomer Royal, I prepared a paper giving the distribution in latitude of all spots from 1874 to 1902, arranging them in zones of one degree wide of heliographic latitude. At the same time I prepared a number of diagrams which were not then submitted to the Society, but which are given in the present paper. Some of these are intended to show the differences of behaviour of the two hemispheres of the Sun, north and south. Others were meant to show the effect of arranging the spots in zones of five degrees wide instead of one degree. For many purposes this gives a truer representation of the actual distribution of spots on the Sun than zones of one, two, or three degrees can do. For though some 5000 spot-groups are included in this discussion, some 20 or 25 are so much larger than the rest that they completely disturb the annual means. And as these great groups have a breadth of five or more degrees, in one case of more than ten degrees, an erroneous effect is produced if the entire area of the group is assigned to the one-degree zone in which the centre of the group may lie. These great explosions are the cause of the chief irregularities of the latitude-curves. Dr. Lockyer, in his recent paper on "Sun-spot Variation in Latitude," is entirely mistaken as to the real nature of the maxima which he obtained by arranging the Greenwich results in three-degree zones. He is further mistaken in joining these unreal maxima so as to form downward-moving "spot-activity tracks." This joining up is entirely arbitrary, and there are no such "tracks" on the Sun as Dr. Lockyer supposes. My last slide shows the distribution in latitude, rotation by rotation, of all spot-groups from 1877 to 1902, and demonstrates that there is but a single zone of spot-activity in each hemisphere, and that that zone moves in precise accordance with Spörer's law. Spörer's law is only deficient in that it does not notice a tendency in the outer limit of latitude to rise at the beginning of a sun-spot cycle, and the possibility of the drift downwards in one hemisphere crossing the equator and entering the other hemisphere.

Rev. A. L. Cortie. The present paper has been prepared in view of recent criticisms on Spörer's law of the variation in latitude of sun-spots. It is perfectly independent of Mr. Maunder's paper nor has there been any collusion between Mr. Maunder and myself in the preparation of our respective papers. It consists mainly of the discussion of a chart on which the mean heliographic positions of 123 greater solar disturbances, representing 328 different group-numbers, observed at Stonyhurst during the period 1881-1903, have been plotted. The first part of Spörer's law concerns the gradual decline in the mean latitude of the spots from $\pm 30^\circ$ to $\pm 5^\circ$ with the fall of the solar cycle from maximum to minimum, and as to the general truth of this part of the law most solar observers are agreed, taking the term mean latitude in a wide sense. The law further discloses that a second solar cycle begins abruptly by the appearance of spots in high latitudes at the time of minimum. Dr. Lockyer's interpretation of the phenomenon of variation in latitude is that a succession of "spot-activity tracks" appear successively, with their starting points continually rising in latitude as maximum is approached, but the trend of the tracks being all downwards—from high to lower latitudes. My own interpretation of the points plotted on the chart [screen], and the corresponding disturbances discussed in the paper, is that, so far as greater disturbances are concerned, there is a single wave of "limiting spot-latitude," which descends from maximum to minimum, but at or about the time of minimum begins to rise again to a second maximum, the greater disturbances being highest in latitude at the times of maximum. Disturbances in high latitudes are not confined to times of solar minimum spot-activity, but are to be found at most periods, while at times of maximum disturbances occur in all latitudes. In drawing curves of mean latitude of sun-spots it seems to me that regard ought to be paid to the area of the spots, for otherwise a mean position is given, for instance, of even a very small spot observed in latitude -40° at Stonyhurst in 1889, and the greater June spot, the biggest of the same year, in latitude -7° , which is quite fictitious. Some system of weighting ought to be adopted if mean latitude-curves are to be drawn. But in the chart we are only concerned with the greater disturbances, which are considered to be the indices of the positions of greatest solar activity, so far at least as sun-spots are concerned. Accordingly two smoothed curves of "limiting latitude" have been drawn, one for the northern, and the other for the southern hemisphere, which contain all these greater disturbances within their contours, and show how the disturbances rise, not abruptly, but gradually from minimum to maximum for the period discussed. This interpretation would substitute one long rising and falling wave of "limiting latitude" for Spörer's two overlapping falling waves of "mean latitude," and though joining the largest points of Dr. Lockyer's "spot-activity tracks" would demand that they should trend upwards from lower to

higher centres in the period minimum to maximum. These instances shown on the chart, in one case a chain of associated disturbances having lasted for 527 days in almost the same position, it would appear that the outbreaks of spots are rather zonal, and intermittently so, than of the form of storm-tracks. It seems difficult to connect by tracks regions of long-continued activity in which all the disturbances are linked together, and also regions of long-continued dearth of spots*.

Dr. W. J. S. Lockyer. I am glad to think that the paper I published some time ago has had the effect of stirring up workers to discuss sun-spot circulation. The main point I wished to draw attention to in my paper was that we still regard this circulation as taking place strictly according to the law put forward by Spörer, disregarding the great amount of fresh material at our disposal. I am glad that Father Cortie is with me in regarding Spörer's law as giving an excellent idea of sun-spot circulation *generally*. Anyone who, like myself, has analysed curves showing the distribution of sun-spot area per degree of solar latitude for each year, will see that we are not dealing with one belt of spots with one definite maximum region of activity in it, but that there are two and sometimes three maxima in this belt. Father Cortie agrees with me in saying that sun-spots occur in highest latitudes about sun-spot maxima, but he regards the curve representing the variation of the limiting parallels of latitude from sun-spot minimum to maximum as being a gradual and not an abrupt rise. On the slide before you [screen] I have drawn a curve joining the points of commencement of the spot-activity tracks, and I should say that at these times in question the curve is more abrupt than otherwise. As to the meaning of these spot-activity tracks, it must be understood that they are not tracks on the solar disc, as, for instance, might be ascribed to that of the proper motion of a spot, but that they are the changes of position as regards latitude year by year of those zones in which the greatest spot-activity takes place. If the maxima of the yearly curves previously described be joined up, the general trend of all these lines is from high to low latitude. The discussion of the whole series of 42 curves in this manner indicates that the whole trend from first to last is of this character.

* [Note.—Owing to limitations of time at the Meeting one important section of the digest of my paper was omitted, which I may perhaps be permitted to resume in this note. It is that these curves of "limiting latitude" are very similar to those of mean sun-spot area, and show that sun-spots flow to higher and higher latitudes as they become greater and more important. This is in accordance, too, with the flow of solar prominence centrals according to the curves drawn by Sir Norman and Dr. Lockyer, and with the gradual opening out of the coronal streams as maximum is approached. All these phenomena would therefore seem to be connected. On this view, too, magnetic storms on the Earth are connected with a generally-disturbed state of the Sun at times of maximum, and not with any particular phenomena of spots or prominences.—A. L. C.]

Father Cortie has pointed out, in an abstract of his paper which he kindly forwarded to me, that from minimum to maximum the spots increase in latitude simultaneously with the approach of prominences to the poles and the existence of polar coronal streamers. This is a point which I am very pleased to see corroborated by him. He has also mentioned that the spot-activity tracks instead of approaching the equator from minimum to maximum, should really recede from it. For the epochs mentioned I have tried to draw the curves in this manner, but the trend towards the equator is far more apparent than away from it. Another point of importance which Father Cortie mentioned is that sun-spots occur in zones and intermittently in them. This, of course, is so. In my discussion I have treated the solar disc as divided into zones of latitude 3 degrees broad, and dealt with the spots in them regardless of their distribution in longitude. If longitude be included, then this is entirely another piece of work. This intermittent distribution of spots in longitude is equally prominent in pictures of the Sun taken in "K" light with the spectro-heliograph where single masses of flocculi are the rule and not the exception.

The following papers were announced and partly read:—

W. F. Denning. "The Rotation Period of Saturn."

P. H. Cowell. "Analyses of Errors of Moon's Longitude for Inequalities of Longer Periods: Methods and Results."

Mr. E. J. Gheury. "Note on the Gyroscopic Collimator of Admiral Fleurbaey."

Rev. A. L. Cortie. "Variation in Latitude of the Greater Sun-spot Disturbances, 1881-1903."

Bryan Cookson. "The Mass of Jupiter, and Corrections to the Elements of the Orbits of the Satellites, from Heliometer Observations made at the Cape in 1901 and 1902."

P. H. Cowell. "The Parallax Inequality: a Reply."

W. E. Plummer. "Cometary Observations at the Liverpool Observatory, 1902-1903."

E. W. Maunder. "Note on the Distribution of Sun-spots in Heliographic Latitude."

Royal Observatory, Greenwich. "Micrometrical Measures of Double Stars made with the 28-inch Refractor in the Year 1903."

A. R. Hinks. "Reduction of 290 Photographs of Eros made at nine Observatories; with a Determination of the Solar Parallax."

The following Foreign astronomers were elected Associates of the Society:—

Henri Deslandres, D. ès Sc., F.R.A.S., Observatoire d'Astronomie Physique, Meudon, S.-et-O., France.

C. D. Perrine, Lick Observatory, Mount Hamilton, California, U.S.A.

George W. Ritchey, Yerkes Observatory, Williams Bay, Wisconsin, U.S.A.

The following gentlemen were elected Fellows of the Society:—

Edward Barlow, Harewood, Andover, Hants.

Otto J. Klotz, Department of the Interior, Ottawa, Canada.

Major C. Leigh-Lye, 15 Mayfield Avenue, Chiswick, W.

The following Candidates were proposed for election as Fellows of the Society:—

William Allan, M.A., B.Sc., 88 Leamington Terrace, Edinburgh (proposed by P. S. Hardie).

Ernest Cuthbert Atkinson, M.A., formerly Assistant-Curator of the Temple Observatory, Rugby, Erwood, Beckenham, Kent (proposed by H. H. Turner).

Arthur Henry Bagnold, Colonel R.E., Superintendent of the Building Works Department, Royal Arsenal, Warren Road, Shooters' Hill, S.E. (proposed by Major P. A. MacMahon).

Rev. D. B. Marsh, D.Sc., Hamilton, Ontario, Canada (proposed by John A. Brashear).

William Newbold, Tonbridge School, Tonbridge, and 7 Broadwater Down, Tunbridge Wells (proposed by Edward Weldon).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1904 May 25.

President: S. A. SAUNDER, M.A., in the Chair.

Secretary: A. C. D. CROMMELIN, B.A.

Mr. A. C. D. Crommelin read the list of presents received, and the thanks of the Meeting were accorded to the respective donors.

The election by the Council of one new member was unanimously confirmed.

Mr. Crommelin read a paper on "Mock Suns" by *Miss C. O. Stevens*, describing an unusually fine display of this phenomenon on April 20 from 5^h 10^m to 6^h P.M. The writer noticed, as a matter of interest, that the general temperature of Ireland on that day was 20 degrees colder than in London.

Mr. C. Thwaites said that he saw the mock sun referred to between 5.30 and 6 P.M.

Mr. T. W. Backhouse, in reply to a question as to the possibility of photographing such a phenomenon, said he had done so, but it did not come out with the distinctness on the photograph with which it was visible to the naked eye.

Mr. J. A. Hardeastle asked what the radius of the halo was. He had often measured them and had always found them between 22 and 24 degrees.

Mr. Crommelin said that the mock sun described by Miss Stevens was evidently the same one that was seen at the Royal Observatory, Greenwich. Miss Stevens used the term rainbow, but she evidently referred merely to the coloured band.

The President said that a halo of that sort was formed by the refraction of sunlight through ice-crystals of a particular shape, in the air, all more or less in the same direction, and the angle of the halo depended on the angle at which the different faces of the ice-crystal were inclined to one another, which was a constant angle.

Mr. W. T. Lynn read a paper on "The Total Eclipse of 1904," suggesting that the opportunity of observing the eclipse from Christmas Island, where the duration of totality would exceed five minutes, should not be missed. He remarked that Captain Cook, the discoverer of this island, had observed the eclipse of the 30th December, 1777, from it.

Mr. Crommelin said he was rather surprised that Mr. Lynn made out Christmas Island to be within the shadow-track. He himself had drawn a track on a map of the Pacific, and that placed Christmas Island outside the totality track. Possibly there were discordant positions given for this island *. The eclipse ought, if possible, to be observed, as it would represent a stage in the solar cycle where very little material existed. He thought it would be quite worth while for the Americans to try to do it from a ship.

The President said that the Council had appointed a Committee to consider the question of forming an expedition to observe the eclipse to be seen in August next year. Mr. Chambers, who was a member of the Committee, would read a translation of a letter he had received from the Director of the Madrid Observatory.

Mr. G. F. Chambers read the letter in question, in which the writer stated that, although the temperature of the northern coast of Spain would be lower than the southern coast at the date of the eclipse, the temperature of the south coast would be quite supportable for English people. In regard to a clear sky, the probabilities were in favour of the southern coast. At the northern end of the central line it would be prudent to keep some distance from the coast. The Spanish Government would probably, as on the last occasion, concede exemption of duties on the instruments, and the railway companies would probably issue reduced-fare circular and other tickets. Mr. Chambers added that the statement as to the summer temperature on the southern coast was in strong conflict with the information he had obtained from other sources, which was to the effect that in the month of August the

* Subsequent examination shows that the island is evidently outside the track of totality.

heat on the Mediterranean coast of Spain was altogether intolerable.

Mr. Chambers, in reply to inquiries, said he could not say what temperature should be described as intolerably hot, but it was pretty clear from the letter just read that observers would run a certain risk of bad weather among the mountains in the north of Spain, while those who could face the discomforts of an extreme temperature had a great guarantee of success with regard to the Mediterranean coast of Spain.

Mr. Backhouse suggested the Balearic Islands as suitable points for observation.

Mr. Chambers replied that the central line went between rather than over them, and that the difficulty of access was a serious question.

Mr. A. Fowler gave an account of his recent identification of the flutings which appear in the spectra of Antarian (3rd type) stars, illustrated by photographs. He remarked that among the varieties of stellar spectra recognized by Secchi there were two in which the most striking features were shaded bands or flutings. These constituted his well-known 3rd and 4th types, which were afterwards carefully observed by Dunér and others, and a good idea of the appearance of these spectra could be gathered from the drawings which were published by Dunér in his classical memoir. In the 4th-type stars there were three prominent dark flutings, sharp towards the red and shading off towards the violet. Secchi himself produced evidence to show that these originated in the vapour of carbon or one of its compounds, and this had since been abundantly confirmed. In the 3rd-type stars, however, the flutings graded off towards the red, and no satisfactory identification had been made until the present year; owing to the effects of contrast it had in fact been uncertain whether the spectrum consisted wholly of dark flutings fading towards the red, or partly of bright ones fading the opposite way. The speaker's own investigations indicated that the spectrum was essentially an absorption spectrum, and that nearly all the dark flutings were produced by the vapour of titanium or its oxide. Further experiments were in progress with the view of ascertaining whether the absorption was produced by the metal or by its oxide. It was of special interest to find that in the spectra of sun-spots the lines of titanium were among those which were most broadened and darkened, and it might be that in the stellar spots, if they existed at all, there was also vapour of titanium which was sufficiently reduced in temperature to produce the dark flutings.

Mr. Evershed asked if the bands sometimes seen in sun-spot spectra had any relation to the titanium bands.

Mr. Fowler said he had not gone thoroughly into that point, but the first comparison would seem to show that they were not the same bands.

Mr. Hardcastle asked if flutings were a characteristic of the

titanium spectra, or whether that spectrum would give lines under other conditions.

Mr. Fowler replied that a substance like titanium might give a line or a fluted spectrum according to the condition of the experiment, or probably according to the temperature of the vapour. It was usually understood that a fluted spectrum was one produced where a vapour was at a low temperature.

Mr. Backhouse asked whether "fluted" meant simply a gradual shading off, or whether, under a higher power, it would show a series of lines getting fainter and fainter.

Mr. Fowler said that was the right idea of a fluting, but the structure was only seen, usually, when the dispersion was considerable.

Mr. Crommelin asked if that applied to the carbon flutings as well.

Mr. Fowler replied in the affirmative.

Mr. A. J. S. Adams read a short paper entitled "Remarks upon Magnetic Disturbances" (illustrated). The object of the paper was to show

- 1st. That the Sun was the sole source for these disturbances.
- 2nd. That such disturbances, whether great or small, were differences in degree of one and the same thing.

The illustrations were taken from the Royal Observatory Records, reproduced as lantern-slides.

Mr. Crommelin gave some particulars regarding Comet 1904 *a*. He thought that the parabolic elements by Herr Ebell, published in the last issue of the 'Journal,' were not very far wrong. They made the perihelion passage to have taken place on the 28th February, and the inclination of the orbit to the ecliptic 55 degrees; that was to say, it was a retrograde orbit, and inclined 55 degrees to the ecliptic. A very interesting point about it was the very large perihelion distance, which was 2.688 in terms of the Earth's mean distance from the Sun. Giacobini's Comet of 18 months ago had a similar perihelion distance, which at the time was the largest recorded with the exception of a comet discovered about 150 years ago.

Mr. Crommelin then gave an abstract of a paper by *Mr. C. T. Whitmell* on "Orbits," and the Meeting adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE second of the afternoon Meetings for the present Session was held on Wednesday, June 15, in the rooms of the Society, 70 Victoria Street, Westminster, *Capt. D. Wilson Barker*, President, in the Chair.

The *Rev. C. F. Box*, gave an account of some curious "Effects of a Lightning Stroke at Earl's Fee, Bowers Gifford, Essex, April 13, 1904." A thunderstorm occurred during the early morning hours, and about 3 A.M. there was a blinding flash, lighting up the whole neighbourhood for miles around, followed immediately by a crashing explosion. One person stated that he saw what appeared to be a cylinder, and another person a ball of fire descend and then explode, "casting darts" in all directions. On careful examination in daylight, it was found that in an oat-field, which had recently been dredged, there were three distinct sets of holes ranging from nine inches down to about one inch in diameter. The holes, which were perfectly circular, diminished in size as they went downwards, and remained so on to the perfected rounded ends at the bottom. Upon digging sectionally into the soil, which is stiff yellow clay, it was found that the holes were "as clean cut as though bored with an auger." An interesting discussion followed the reading of this paper.

A paper by *Mr. A. Lawrence Rotch*, of the Blue Hill Observatory, U.S.A., describing "An Instrument for determining the true Direction and Velocity of the Wind at Sea," was, in the absence of the author, read by the *Secretary*.

Ancient and Modern Ideas about the Milky Way.*

THE name of the Milky Way recalls one of the most ancient of the legends that surround it. The goddess Juno, at the request of Jove, would have suckled the infant Hercules, but the strong and turbulent nursing refused the refreshment offered him, and the gushing milk flowed amongst the fixed stars.

But this explanation, it may well be believed, is not very satisfactory to the man of science. Some have considered the Galaxy as the plan of the journey once followed by the chariot of the Sun, doubtless when the reins were restored to the unskilful hands of Phaeton. More prosaic, Theophrastus saw in it simply the mark of the imperfect soldering of two hemispheres; but the first opinion which deserves our consideration is that of Democritus, the philosopher, as well known as his works are few, who lived at Abdera 300 years before the Christian era. In a short fragment quoted by Plutarch, he declares that the Milky Way is an agglomeration of small stars too far away to be perceived singly. This remarkable intuition was generally neglected or treated as badly-founded conjecture, and it was reserved for Galileo, who came 2000 years after, to restore it to honour. In his 'Sidereus Nuncius,'

* Translation of an article by M. P. Puiseux in the *Bulletin Astronomique*, for May 1904 (by permission).

published in 1610, he takes up the hypothesis of the philosopher of Abdère. He makes it his own, and presents it as an incontestable result, furnished by the recently invented telescope. "At last," cried he, "we have solved all the difficulties which have tormented the philosophers of all ages; we are saved from wordy disputations."

The observation of the learned Florentine was, in fact, excellent. He had recognized that in certain parts, at least, the diffused whiteness which meets our gaze resolves itself in the telescope into little brilliant points strewn upon a black ground. Nevertheless, Galileo shouted too soon; it was inevitable that crucial questions should be put to him. Why this capricious distribution, why this superabundant richness next to regions bare of stars? Gassendi, while noting the difficulty, gave up the attempt at solution. "If some one," he said, "wishes to know why this cluster of small stars is arranged in a circle rather than in any other form, let him consult the Creator of the stars, the Sovereign Being who made them and arranged them as it pleased him, and who alone knows his work."

If this counsel had been followed, our study would be ended. Fortunately the astronomers of the eighteenth and nineteenth centuries have been less easily satisfied than Galileo, less quickly discouraged than Gassendi. The study of the Milky Way has supplied them with the opportunity to write some of the most attractive chapters of the science, and in the last twenty years celestial photography has supplied an imposing mass of evidence which has entirely resuscitated the subject.

To follow this evolution of the ideas of astronomers, it is convenient, in the first place, to take a general glance at the phenomenon. For this purpose one need not arm himself with a powerful telescope which covers only a small field. The observation must be made with the naked eye in a clear sky, and the observer must be able to transport himself at will from the north to the south hemisphere. And if one wishes to call photography to his aid it will not be a giant object-glass which will render the best service: a lens of modest aperture and short focus will be preferable for first examination.

Many works, Arago's 'Popular Astronomy' for example, contain drawings which show the plan of the Milky Way on the south half and the north half of the celestial sphere respectively. There exist more modern and more detailed representations by Heis, Gould, and Bøddicker, but their richness prevents one from seeing certain general features which are the most important and which have been recognized long ago.

In the southern hemisphere we see our luminous trail pass over the constellations of Monoceros, Argo, the Southern Cross, Centaurus, Scorpio, Sagittarius, Ophiuchus. In this portion one can note ramifications, increases of brightness, lacunæ; in the Southern

Cross particularly an absolutely empty space, thirty degrees square, the blackness being still further accentuated by a surrounding of bright stars, which has long been known to sailors and is called by them the *Coal Sack*. In the neighbourhood of α Centauri the Galaxy divides itself into two branches of unequal intensity, which pursue nearly parallel paths across the equator. Crossing the equator we find in our path the Serpent, the Eagle, the Arrow, before coming to the Swan. There the two branches join again after a separation of nearly 120° , and quite near one sees a very black space—the Coal Sack of the northern hemisphere. There is yet to be traversed Cepheus, Cassiopeia, Perseus, and Gemini before returning to the starting-point. At several points the Milky Way seems to put forth branches, quickly fading and difficult to follow. Another branch goes to join up with the group of the Pleiades. Despite these irregularities, the general outline is quite that of a great circle of the sphere; and if one estimates from the most brilliant portions, the pole can be determined with a high degree of precision.

The brightest portions are met with in Scorpio and Sagittarius, and so present a spectacle to the inhabitants of the southern hemisphere rather than to us. There can be seen in Scorpio a curious structure—a kind of network of crossing laths. A similar appearance is often presented by the cirrus, light clouds formed by crystals of ice in the upper regions of the atmosphere. In the constellation Cygnus the Milky Way has fields of stars of marvellous richness, resolving themselves, when seen by a powerful telescope, into veritable showers of gold. The same sight is seen elsewhere, though not so pronounced, but between the different parts of this magnificent girdle there are no systematic or radical differences.

It need scarcely be added that these appearances are the same for all the inhabitants of the Earth, that they do not change in the course of a year, and they have not experienced any appreciable variation since ancient times. So that the Milky Way is not a phenomenon atmospheric or local. Its dimensions are enormous in comparison with those of our globe, very large even beside the diameter of the Earth's orbit. It comprises vastly more space than the Sun and his family of planets traverse in several centuries.

From this consideration we have no means of understanding the structure of the sidereal world from change of place of observation. We must content ourselves with the position that has been assigned to us. Must we wait before hazarding an interpretation of the appearance presented until a detailed study of proper motions and parallaxes has been made for the majority of the visible stars?

At the time of Galileo and of Gassendi some proper motions had been surmised only. Nothing was known of stellar parallax, and the instruments of that epoch would not have permitted any attempt at such a research. The illustrious Kepler, however, in

his 'Epitome,' published in 1618, made one more step. From the circumstance that the parts of the Milky Way diametrically opposite did not differ much in brightness and size, he inferred that the Sun was at about the centre of the band. This opinion holds its ground until to-day, but it is exaggerating this view to think, as has been done recently, that this is a proof that the Earth and Man occupy a privileged position in the Universe.

To come to some original views upon the same subject, it is necessary to leap a century, and so arrive at the English astronomer, Thomas Wright. In a book, copies of which are to-day almost impossible to obtain, Wright sets forth the bold opinion that the luminous bridge thrown across the sky has no real existence. He thought it to be simply the total effect of our eye taking in at one glance a very great number of stars—not because the stars themselves are actually more concentrated, but because the universe of stars of which we form part is in the shape of a comparatively thin slab or of a flat cake. As we look in the direction of the rim of the disc it is obvious that we should perceive many more stars than if we turned to the upper or lower face.

Whether true or not, Wright's hypothesis had one essential merit, namely, that it lent itself to experimental verification and suggested new researches. But it was not by this practical reason that it charms. One consideration specially strikes us: it gives the Sun, and our Earth with him, a *locus standi* in the Stellar Universe. Henceforth the Galaxy is no longer a distant object, strange, inaccessible. We are immersed in it; we live in it. Its feeble glimmers represent an immense number of solar systems like our own, and from one end to the other of this vast whole the same physical laws exert their sway.

[To be continued.]

Fireball Epochs during Last Half of Year.

I RECENTLY undertook an examination of the dates of 356 large meteors observed by me at Bristol from July to December during the years 1866 to 1904. I also compared and assorted the dates of about 400 fireballs recorded by various observers in the British Isles during the 10 years 1894 to 1903. From these materials I ascertained the nights or periods of special frequency, and have summarized the several results in the following columns. For purposes of comparison I have added the chief dates of Fireballs as I derived them from an examination of all the objects recorded in various catalogues down to 1878 (*Observatory*, September 1879). The number included in the latter investigation totalled 2467 for the last six months of the year.

The results present a pretty fair agreement considering the

different and independent character of the materials employed. The principal meteoric epochs of July, August, October, November, and December are strongly indicated, with the exception perhaps of that of October.

The marked prevalence of brilliant meteors at the end of July is due to the δ Aquarids and early Perseids, but meteors generally are pretty numerous at this particular time of the year. Possibly the same pair of showers are chiefly responsible for the closely following epoch of August 2-4.

There is also an abundance of fireballs on August 19-22, due probably to the α Draconids ($291^\circ + 60^\circ$), which formed an unusually rich shower in 1879, but does not appear to have returned since that date ('Monthly Notices,' xl. p. 127). There are many Cepheids also visible at this period.

September 1, 6-7, and 25 have yielded a plentiful supply of fireballs with apparently discursive flights, so that a number of different showers have been implicated. There are many Cygnids and Cepheids at the two former epochs, and β Piscids at the latter.

Fireballs frequently appear on the first few nights of November, and particularly on November 1; they are chiefly Taurids from a radiant at about $58^\circ + 9^\circ$, and form a distinct system to the strong shower of bright November Taurids at $63^\circ + 22^\circ$. This pair of showers furnishes many splendidly luminous meteors in the evenings at the middle of November, and are in part contemporary with the Leonids (see *Observatory*, 1901 January).

Special watches should be maintained for brilliant meteors on the particular dates alluded to, and during such observations it will be advisable to record the small as well as large objects that may become visible. To simply await the apparition of a fireball must prove rather a tedious, if not unproductive, process; and the noting of faint meteors will serve to entertain the observer as well as furnish useful materials. It is certain that many meteoric systems are formed of large and small objects mixed together, and that a few insignificant shooting-stars will sometimes accurately indicate the radiant of a fireball possibly observed on the same night. But certain large meteors appear to be isolated or to belong to very attenuated streams, of which only one or two fragments may come under notice during a long watch.

It is hoped that a greater number of observers will occupy themselves in looking out for meteors and suitably recording them. Fireballs often come in the early evening or during moonlight, in fact these bodies are apt to appear when least expected. As a natural result they are often accidentally witnessed, and if the fortunate spectators always furnished detailed reports of such events, we should soon amass a large amount of new and valuable data in this department.

356 Fireballs observed by W. F. D., 1866 to 1904.	400 Fireballs by various observers, 1894 to 1903.	2467 Fireballs by various observers, 1492 to 1879.	Showers.
July 8	July	July 11	} Scorpiids, Cygnids, Aquilids.
.....	July 12-15		
21-22		20-21	
24			} δ Aquarids, Perseids.
27-30	27-31	25-30	
Aug. 2	Aug. 2-5	Aug. 3-5	
7-12	8-12	7-13	} Perseids.
15		15	
18-21	19-22	19-22	
25-26	24-27		{ α Draconids, Cepheids.
Sept. 1	Sept. 1-2	Sept. 1-2	
6-7		6-7	
15	15-17	11-13	} α Aurigids, ϵ Perseids. β Piscids.
25	24-28	25	
.....	Oct. 1-4		
Oct. 7-8	8		} ϵ Piscids.
11-12	10-12		
13-17		Oct. 13-15	
19-21	25	24	} Orionids.
.....		29	
Nov. 1	Nov. 1-6	Nov. 1-2 & 4	
.....	9	6-9	} Taurids. Leonids, Taurids. Andromedids —
13-15	13-16	11-15	
.....		19	
26-27	25 27	27	} Geminids.
.....	Dec. 1-2		
Dec. 12	9-12	Dec. 8-9 & 11-12	
.....	16-18		
21-23		21	

Bishopston, Bristol,
1904, April.

W. F. DENNIN ~~██████~~ G.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Longitude and the Magnetic Variation.

GENTLEMEN,—

About the year 1721 it occurred to a medical practitioner named Zachariah Williams, residing at Rosemarket, near Hereford, in Pembroke-shire, that the long-desired problem of finding the longitude at sea might be solved by observing the magnetic variation. He was aware that the declination was subject to change (which, he says, was discovered by Gellibrand in 1625), but thought that the time had come when these could be predicted

and tabulated. Possessed with the idea that he had made a great discovery, he came to London a few years afterwards, and laid his scheme before the Admiralty. It was referred to Newton, who declined to pronounce an opinion upon it on account of his great age. (By an extraordinary blunder, the date of this given in Williams's book is 1729, two years after the death of Newton.) Reference was then made to Samuel Molyneux, whom Williams accuses of surreptitiously copying his scheme in order to keep it for his own use; at any rate, as is well known, Molyneux died little more than a year after Newton. From Halley, Williams says, he received encouragement to publish his tables. He continued to importune the Admiralty to have his scheme examined, his letters being written or corrected by Dr. Johnson (whose kindness to his blind daughter, Miss Williams, is well known to readers of Boswell), and at last reference was made in 1751 to Bradley, then Astronomer Royal, whose report runs thus:—

“Dr. Bradley says that he had compared Mr. Williams's tables with the best observations; that, in some cases, they agreed pretty exactly, but in others the difference amounted to ten, fifteen, or twenty degrees; that Mr. Williams shewed him a magnetical instrument by which, as he supposed, the tables were constructed; that Mr. Williams concealed the principles upon which it was made, nor would allow him to see the internal construction of it; that, upon the whole, as his tables can only be proved by comparing them with observations, and in several cases the difference was so very great, he did not think that the instrument, in its present state, could be relied upon at sea.”

Williams, however, subsequently published his tables with an introduction said to have been composed by Johnson, and an Italian translation by Baretti. The date of this is 1755, and the author died the same year, aged about 80. His daughter, Anna Williams, who had become blind in 1752, died at Johnson's house in Bolt Court in 1783.

Williams's tables profess to give the magnetic variation for nine stations (Lisbon, Dublin, Edinburgh, London, Paris, Rome, Prague, Upsala, and Cracow), from the years 1660 to 1860, but not to any great accuracy. For London his numbers are as follows:—

1660 $0\frac{1}{2}^{\circ}$ W.	1730 $15\frac{3}{4}^{\circ}$ W.	1800 29° W.
1670 2	1740 $17\frac{1}{2}$	1810 29
1680 $4\frac{1}{2}$	1750 20	1820 29
1690 7	1760 $22\frac{1}{2}$	1830 $29\frac{1}{4}$
1700 $9\frac{1}{4}$	1770 25	1840 $29\frac{1}{2}$
1710 $11\frac{1}{2}$	1780 27	1850 $29\frac{3}{4}$
1720 $13\frac{3}{4}$ W.	1790 28 W.	1860 30° W.

It will be a sufficient comment on the accuracy of these predictions to quote from the Astronomer Royal's Report for 1861:—
“The mean magnetic declination for 1860 is about $21^{\circ} 14' 20''$ (9' less than that for 1859).”

Still the question may remain whether improved knowledge of the Earth's magnetism and its lines and changes of variation may make a scheme practicable now which quite failed in 1755. Taking into consideration, however, the changes produced by magnetic storms, of which nothing was then known, I should imagine that Bradley's verdict will still hold good. Yours faithfully,

Blackheath, 1904, June 1.

W. T. LYNN.

The Mobility of the Earth and its First Teacher.

GENTLEMEN,—

It is still an idea largely held that the mobility of the Earth was first taught by Pythagoras, and that in teaching it we are called upon, to use the expression of Copernicus, "*Pythagoreorum et quorundam aliorum sequi exemplum.*" As a matter of fact, however, there is more of myth than of history in the accounts of Pythagoras and his teaching. That he was a remarkable man in his day we need not doubt, and the reverence of his disciples for anything attributed to him, expressed in the words *αὐτὸς ἔφη*, has become, in the Latin translation, "*ipsedixit*" (contracted into one word), an English colloquial term for reliance on the authority of another. That he was a vegetarian may be accepted, though it is difficult to account for his refusal, according to a famous line in Horace, to eat beans. But as regards the respective positions of the Sun and the Earth in the cosmos, there is no reason to suppose that he anticipated Copernicus. Archimedes, in a work to which Copernicus had not access (for it was first printed in 1544, the year after his death), called *Ψαμμίτης*, or in Latin '*Arenarius*,' states that, according to the doctrine of Aristarchus, the diameter of the Sun was nearly thirty times that of the Earth, and that the former, not the latter, was the centre of motion, the Earth being carried in a circle round the Sun, whilst the fixed stars remained immovable.

Aristarchus was probably a few years younger than Archimedes, and may have survived him, the latter having, as is well known, been killed at the siege of Syracuse by Marcellus, B.C. 212.

Yours faithfully,

Blackheath, 1904, May 3.

W. T. LYNN.

Systematic Motions of the Fixed Stars.

GENTLEMEN,—

I have lately made some remarks on the uncertainty of the Sun's motion owing to certain systematic motions among the fixed stars. I have so far traced these in R.A. only. That they occur in Declination also is very possible, but on that point I have nothing to say. I give you the results which I arrived at from an examination of M. Bossert's Catalogue in the '*Annals*' of the Observatory of Paris, which is the largest Catalogue of stars with

considerable proper motion that I have met with. I added them in pages of 50 stars each, which was not very scientific, but in the following table I indicate the point where each page terminates. I need hardly point out that, neglecting the declination of the apex of the Sun's way for the present, and treating only of its Right Ascension, the Sun's motion will make all the stars apparently recede from that point; but recession in one direction will increase the R.A., while recession in the opposite direction will diminish it. A similar remark applies to the anti-apex. All the stars will appear to approach it; but approach from one side means increasing R.A., and from the other side diminishing R.A. The following tables give the increasing Right Ascensions, diminishing Right Ascensions, and neutrals (motion in Right Ascension = 0) for each page of M. Bossert's Catalogue:—

Page	ends	h	m	+	-	0	Page	ends	h	m	+	-	0
" 2	"	0	27 ...	40	8	2	" 9	"	3	46 ...	25	17	8
" 3	"	0	44 ...	34	13	3	" 10	"	4	15 ...	34	12	4
" 4	"	1	3 ...	32	17	1	" 11	"	4	38 ...	36	9	5
" 5	"	1	27 ...	34	15	1	" 12	"	5	12 ...	36	8	6
" 6	"	1	50 ...	30	14	6	" 13	"	5	53 ...	32	8	10
" 7	"	2	15 ...	37	11	2	" 14	"	6	22 ...	24	15	11
" 8	"	2	48 ...	38	11	1	" 15	"	7	0 ...	19	22	9
" 8	"	3	15 ...	37	8	5							

Here I pause, for we have evidently passed from the region in which increasing Right Ascensions predominate to that in which diminishing Right Ascensions preponderate. Where was the passage effected? I think almost exactly at the foot of p. 14, or at R.A. $6^h 22^m$, for the last five stars on p. 14 are increasing their R.A., while the first five on the following page are diminishing it. We pass at a stroke from the one region to the other, and there is no turning back. But a very different state of things exists when we approach the other critical point, about 12 hours later. I resume the table:—

Page	ends	h	m	+	-	0	Page	ends	h	m	+	-	0
" 16	"	7	34...	12	28	10	" 29	"	12	56...	13	34	3
" 17	"	8	13...	18	30	2	" 30	"	13	22...	13	32	5
" 18	"	8	38...	11	32	7	" 31	"	13	49...	11	35	4
" 19	"	9	9...	12	32	6	" 32	"	14	10...	10	30	10
" 20	"	9	33...	13	29	8	" 33	"	14	33...	12	34	4
" 21	"	10	3...	14	28	8	" 34	"	14	55...	14	30	6
" 22	"	10	21...	8	36	6	" 35	"	15	19...	11	31	8
" 23	"	10	46...	12	33	5	" 36	"	15	41...	17	26	7
" 24	"	11	8...	8	35	7	" 37	"	16	11...	15	29	6
" 25	"	11	29...	10	36	4	" 38	"	16	41...	16	26	8
" 26	"	11	50...	13	32	5	" 39	"	17	5...	12	29	9
" 27	"	12	14...	13	34	3	" 40	"	17	36...	17	26	7
" 28	"	12	36...	8	35	7							

The preponderance of diminishing Right Ascensions is clear and manifest up to this point, but here commences the oscillation that seems to me to indicate that the Sun's motion in space is no longer the only important factor with which we have to deal:—

	h	m	+	-	0		h	m	+	-	0
Page 41 ends	18	13...	18	17	15	Page 44 ends	19	56...	20	19	11
" 42 "	"	18 50...	13	30	7	" 45 "	"	20 30...	25	22	3
" 43 "	"	19 25...	21	25	4						

How can this result be explained? The neutral point seems to be approached as closely as possible in the interval $17^h 36^m$ to $18^h 13^m$, but in the next interval, $18^h 13^m$ to $18^h 50^m$, the preponderance of diminishing Right Ascensions is almost as marked as it had ever been. This preponderance, though much reduced, still exists in the interval $18^h 50^m$ to $19^h 25^m$, and it is only in the interval $19^h 25^m$ to $19^h 56^m$ that we get back to the same neutral condition as in the interval $17^h 36^m$ to $18^h 13^m$. Some invisible but potent cause seems to have put back the hands of the clock by an hour and three-quarters. Even in the interval $19^h 56^m$ to $20^h 30^m$ the preponderance of increasing R.A.s is slight. There must, I apprehend, be some cause which diminishes the R.A. of the great majority of the stars in this region of the sky, and thus counterbalances the effect of the Sun's motion.

From the point at which the above table terminates, all proceeds smoothly to the end. There is only the one well-marked region of disturbance embracing about two hours of R.A. The remaining figures are:—

	h	m	+	-	0		h	m	+	-	0
Page 46 ends	20	54...	27	18	5	Page 50 ends	22	45...	34	16	0
" 47 "	"	21 19...	27	21	2	" 51 "	"	23 11...	34	14	3
" 48 "	"	21 49...	29	16	5	" 52 "	"	23 35...	40	7	3
" 49 "	"	22 19...	33	14	3	" 53 "	"	24 0...	22	17	2

These figures, however, hardly exhibit the amount of preponderance which might be expected in the absence of any counteracting cause—at least the earlier ones do not. Supposing that the stars were moving indifferently in all directions, the heavens ought to be divisible (as regards R.A.) into two regions of 12 hours each, in one of which the observed proper motions of the stars in increasing R.A. preponderated, while in the other decreasing R.A.s preponderated; and the degree of preponderance ought in both cases to be the same. But (and I have tried other Catalogues than M. Bossert's, though I have not the results ready to hand when writing) the two regions into which the heavens appear to be thus divisible do not consist of 12 hours each, nor is the amount of preponderance in each region the same. I doubt if we can arrive at any safe conclusion with regard to the Sun's motion in space until we know something more about the systematic motions among the fixed stars that lead to such results as I have indicated.

Truly yours,

W. H. S. MONOD.

Dublin, 1904, June 15.

P.S.—I noticed elsewhere a remarkable agreement between what I may call the neutral points and the intersections of the Galaxy with the equator. It might almost be said that diminishing Right

Ascensions predominate when the Galaxy is north of the equator, and increasing Right Ascensions when the Galaxy is south of it. The Galaxy is narrow at one intersection; it is wide, and in fact double, at the other. But whether the position of the Galaxy materially affects the systematic motions referred to I cannot say.

OBSERVATORIES.

ROYAL OBSERVATORY, GREENWICH.—Except for one absentee all the members of the Board of Visitors assembled at Greenwich on Saturday, June 4, to receive the Astronomer Royal's report for the year ending 1904 May 10. The Visitors with the Assistants and Established Computers were entertained to lunch in the Octagon Room, and there was no Visitation dinner. The general company numbered about one hundred. The following details are taken from the Astronomer Royal's report.

Transit-Circle.—The number of Transits observed during the year was 13,270, of Circle observations 11,470. This number of transits is the largest ever reported for any twelve months. The stars observed were chiefly the reference stars for the Astrographic Catalogue, of which there are more than 10,000, and the progress has been so satisfactory that it is arranged to end this series of observations at the end of 1905, a year earlier than was originally proposed.

Correction for variation of latitude is to be applied to observations of North Polar distance made in 1902 and subsequent years. Prof. Albrecht has kindly consented to supply the data for this from his provisional results of the international series of observations.

The apparent correction for discordance between the observations of the nadir and stars observed by reflexion for 1903 was found to be $-0''.23$. The correction for the R—D discordance was found to be $+0''.045 + 0''.261 \sin z.d.$, a value agreeing well with the results of recent years. The correction to the tabular obliquity of the ecliptic was found from observations in 1903 to be $+0''.24$. The apparent correction from observations of the Sun to the Right Ascensions of clock stars is $-0^s.024$.

A comparison of the Greenwich meridian observations of the Moon from 1750 to the present time with the lunar places as given by theory is being made. Explanations of the method of analysis have been published by Mr. Cowell in this Magazine and in the 'Monthly Notices of the R.A.S.' The re-reduction of Groombridge's observations is completed.

The Altazimuth.—This instrument has undergone some alterations during the year, the most notable being a readjustment of the object-glass, which appears to have reconciled a discordance which existed between the results of observations in different positions.

The telescope is capable of being reversed in its bearings, and since the stand has a motion of rotation in azimuth, four combinations are possible for meridian observing, and the instrument is used in all four. During the year 1349 observations of the R.A. of the Sun, planets, and stars were made, and 1056 of N.P.D. The instrument is also used out of the meridian, and the azimuth and zenith distance of 319 stars were determined, and 29 of the Sun and planets. The total number of observations of the Moon made at Greenwich during the twelve months is 106 with the transit-circle and 88 with the altazimuth—viz., 35 in the meridian and 53 out of the meridian.

Observations with the *Reflex Zenith Tube* have been made continuously during the year. An elaborate programme of observation has been arranged, and in order to carry out this it is necessary to have morning watches at certain periods of the year. The observations for 1902 have been printed, and the hitherto unpublished observations from 1886 to 1899 prepared for press.

28-inch Refractor.—The total number of double stars measured during the year with this instrument was 512. Of these 178 have their components less than $1''\cdot0$ apart and 85 less than $0''\cdot5$. The wider pairs consist of bright stars with faint companions, stars of special interest, and stars from Struve's catalogue which have not been measured within the last 30 years.

Thompson Equatorial.—This instrument was out of use from September 17 to November 27, whilst the covering of the dome was being renewed. With the 26-inch refractor 58 photographs of Neptune and its satellites and 22 photographs of double stars have been taken. With the 30-inch mirror, which was re-silvered in November, 117 photographs of 40 minor planets and 44 of comets c 1903 and a 1904 have been taken in addition to 16 long exposure photographs of the same comets and other objects. The measurement of the photographs of Eros taken during the Opposition of 1900-1 with this instrument and with the Astrographic Equatorial is proceeding.

The measurement of the Astrographic Catalogue plates has been continued, and there remain only 98 out of 1149 to be measured. Volume I. of the Catalogue has been published and the printing of Volume II. is now in hand. The reproduction of enlarged prints of the chart plates for distribution has been begun.

The solar activity has increased considerably during the year ending May 10, the Sun being free from spots on only 25 days as against 190 in the previous year. The magnetic disturbances in 1903 have shown a marked increase in number and extent. There were 5 days of great magnetic disturbance and 7 of lesser disturbance.

The mean temperature of the year 1903 was $50^{\circ}\cdot2$, or $0^{\circ}\cdot7$ above the average of the 50 years 1841-90. The lowest temperature of the air recorded in the year was $23^{\circ}\cdot8$ on January 1. During the winter there were 43 days on which the temperature fell below $32^{\circ}\cdot0$, being 13 below the average number.

The rainfall for the year ending 1904 April 30 was 35.42 inches, being 10.88 inches greater than the average of the 50 years 1841-90. The number of rainy days was 182. The rainfall during 1903 was 35.54 inches, the heaviest ever recorded at Greenwich during a calendar year. The greatest fall registered at Greenwich on a single day for many years past, viz. 2.43 inches, occurred on July 23. The total fall from 1903 March 1 to 1904 February 29 was over 37 inches.

Mention is made of the retirement, on December 31, of Mr. W. C. Nash, after a service of nearly 48 years. This retirement led to the promotion of Mr. Crommelin and Mr. Bryant to be Assistants in place of Second-Class Assistants, and to the promotion of Mr. C. Davidson to the class of Higher Grade Established Computer.

ROYAL OBSERVATORY, CAPE OF GOOD HOPE.—Sir David Gill's report for 1903 begins by describing the state of affairs relating to certain accessories of the New Transit-Circle. It will be remembered that azimuth marks of a novel kind have been in course of construction, which entailed the making of deep pits lined with cast-iron cylinders. Some trouble has been caused by the attempt to make them water-tight, but they are now thought to be so. The necessary object-glasses of long focus for the marks have not yet been received. A Repsold registering micrometer is to be used on this Transit-Circle, but whether it is to be actuated by automatic means or not, does not appear to be yet decided.

The spectroscope of the Victoria Telescope was received back from the Cambridge Scientific Instrument Company in 1903, May. A description is given of the very ingenious apparatus for keeping this instrument at a constant temperature.

The new sidereal clock has also been received from the same Company at Cambridge. The clock consists of (a) a pendulum swinging in a nearly air-tight enclosure, the air in which is maintained automatically at uniform temperature (30° C.) and low uniform pressure (30 millimetres); and (b), a "slave-clock" with wheel-train pendulum and dead-beat escapement which is controlled by pendulum (a), but gives the latter the necessary impulse, keeps it at uniform temperature, and does all the necessary work for the chronograph, &c.

The work with the Transit-Circle has been confined to the completion of a Catalogue of 2798 selected zodiacal stars, besides stars required for geodetic occultation purposes. The number of transits observed during the year was 6262; of determinations of Z.D. 5711.

The heliometer has been devoted to the observation of the major planets at opposition and of stars in connection with the same work.

In the Astrographic Department the work is proceeding rapidly; 1285 chart-plates have been already taken out of 1632. All the catalogue plates have been taken once, and about three-quarters of

a duplicate series of these is done. During the year 156 catalogue plates were measured in two positions, containing 80,808 stars, which brings the number measured to 586, leaving 926 to be measured. It is estimated that the Cape zone, Dec. -40° to -52° , will comprise about 400,000 different stars shown with six minutes' exposure.

The work with the spectroscope has mainly been preliminary. 104 photographs were taken for focussing and adjustments, but 22 spectrum plates were obtained on thirteen flights. The most remarkable result is the rapid radial velocity of α Phœnicis, which in December was receding at 105 kilom. per second from the Earth.

H.M. Astronomer, at the request of the Colonial Government, has superintended the Geodetic Survey of South Africa, and though the Admiralty, to whom this report is addressed, is not concerned in this work, details of the proceedings are given. The Anglo-German boundary of South-west Africa is completed, and the Report will shortly be presented. The Geodetic Survey of Southern Rhodesia is completed. The measurement of an arc of meridian, 30° east of Greenwich, from the Zambesi to Lake Tanganyika, has been begun by Dr. Tryggve Rubin, who was a member of the Swedish-Russian expedition to Spitzbergen in 1901, 1902. The Geodetic Survey of the Transvaal and Orange River Colony was actively begun in January 1903, under the superintendence of Col. Morris, R.E. In connection with this survey five base-lines will be measured. Three have, in fact, already been measured by the Jaderin method with wires of nickel-steel; and since they have each been measured two or three times, a probable error has been deduced. The lengths of the bases are 10.8, 11.8, and 13.5 miles, and the probable errors of their measurement about one centimetre.

PUBLICATIONS.

THE GREENWICH ASTROGRAPHIC CATALOGUE.—An unguarded remark of the writer, to the effect that the introduction to the Greenwich Astrographic Catalogue was one of the best things that he had ever read, brought on him an invitation to review the recently published volume in these columns. The task is a pleasant one, for the first published British contribution to the great Catalogue has many conspicuous merits.

In the first place, it is complete as far as it goes. The zone in declination $+64^{\circ}$ to $+65^{\circ}$ appears, of course, on only one series of plates; it will be duplicated by the Vatican Observatory. The successive zones northward as far as Decl. $+72^{\circ}$ are found on overlapping series of plates, and are here completely published. The stars in each zone are numbered consecutively right round the sky, so that their numbers can be quoted exactly as the B.D. number of a star is commonly quoted, and the measures of the same star on overlapping plates are printed side by side. Moreover,

care has been taken that stars shall not be measured more than twice, even though they may appear on the edges of other plates which just overlap. The result of this arrangement is that when a star has once been found in the Greenwich Catalogue there is the whole information in one line. No arrangement of separately numbered single plates, zone by zone in separate volumes, can compare in convenience with this scheme, whose execution has been very much simplified by the adoption of the ingenious duplex measuring machine described in the introduction. Astronomers who can think only in R.A. and Declination will complain no doubt at first that it is impossible to find a star in any volume of the Astrographic Catalogue that does not give those co-ordinates at least approximately for every star; but they will find the use of the Greenwich Catalogue an education in rectangular co-ordinates freed from its more forbidding difficulties.

The measures are given in terms of the 5-mm. réseau interval to four places of decimals, so that a unit in the fourth place corresponds to $0''.03$. It appears to the writer that this réseau interval is certainly the best unit to adopt for astrographic work; other observatories, measuring in millimetres, cannot stop at three places of decimals, while one in the fourth place corresponds to $0''.006$, which is too small a quantity to have any real use on any plates which have ever been taken, with the possible exception of recent photographs with the Yerkes 40-inch refractor.

The plate-constants are derived from the stars in the Helsingfors and Christiania A.G. zone catalogues as far as zone 69° , and beyond that from Greenwich observations made specially for the purpose. It is worthy of note that the assignment to Greenwich of the north polar regions brought with it a very heavy burden of meridian observation that could scarcely have been borne by any other than a great national observatory. The reward of this labour is found in the proof that a very large part of the uncertainty in the determination of plate-constants arises from the roughness of meridian star-places, and that the northern zones of Greenwich will have better plate constants than can be determined from the A.G. Catalogue.

The discussion of the constants makes a very interesting section of the introduction. Six constants were first determined in the usual way. A brief computation cleared them of the variable parts due to refraction and aberration, when it appeared that the scale-value might be with all necessary accuracy considered constant. The resulting deduced values of a and e being introduced into the solution, new values of b and d were found which when cleared of refraction and aberration should have been equal, denoting the orientation correction of the plate. The numerical mean of b and d was taken as the real value of both, and introduced again into the equations to find the constants for centre; the values finally adopted for them are presumably the means of the four values for each which would thus be found. The table which gives the differences between the originally found and finally

adopted values of the plate-constants is a mine of information, whose discussion is promised in the third volume. Just enough is given in the first to show that some very interesting things will come out. For example, there is a systematic unexplained discordance between b and d , which looks like an effect of want of perpendicularity of the réseau lines, but is not. Its existence seems to the writer a complete justification of the view which has often been condemned on *a priori* grounds, that the better fit obtained in a reduction with six constants is not a fictitious computation effect, but represents something real though hitherto unexplained. For the discussion of these and kindred matters there is a great deal of material now made available in this volume; and it seems to be a matter for some regret that no indication is given of the stars that have been used for the reduction of each plate to standard—a knowledge of their distribution on the plates may turn out to be essential.

The determination of the photographic magnitudes leads to the interesting result that plates which were intended to include all stars down to mag. 11 contain many below mag. 12. The magnitudes have been found to conform, as far as photographic magnitudes may, to Pickering's photometric scale, by the determination of the two constants C and n in the formula

$$\text{Magnitude} = C - n \sqrt{\text{diameter of image.}}$$

Two groups of stars, fairly bright, and the faintest shown, were selected on a number of plates, the values of these two constants found, and their mean value adopted as a definitive expression of the relation between magnitude and diameter on all the plates. A comparison of the B.D. magnitudes (reduced to photometric scale) with their observed diameters on any plate will bring to light any systematic peculiarity of that plate. But it should be remarked that values of the constants determined on the individual plates discussed show a decided relation to the magnitudes of the selected groups of stars. And it is not shown that the law deduced from groups of nearly limiting magnitudes, bright and faint, fits the stars of intermediate magnitude. It appears to the writer that more information, especially on this latter point, would be of great value.

Space forbids more than a mention of the valuable discussion of errors of personality in measurement, of the errors of the measuring machine, and of the errors of the original réseaux (but not of the probably more important projection errors). The introduction provides a wealth of knowledge of these and many other matters; the body of the catalogue gives the places of nearly 80,000 stars arranged in simple and admirable form; the whole forms a work worthy of the Royal Observatory, engaged for the first time in a great international enterprise. Everyone will desire to offer to the Astronomer Royal and to all the members of his staff sincere congratulations on the conclusion of this, the first part of their labours.

A. R. H.

DIE BILDERZEUGUNG IN OPTISCHEN INSTRUMENTEN *.—This book, which forms the first volume of a series on the theory of optical instruments, is an object-lesson to the man who prefers that things should be done by "rule of thumb" and has a contempt for book learning as distinguished from practice, for it is a splendid treatise on Optics written by the men who have made the firm of Zeiss famous. It is a large book comprising ten chapters, each treating of some branch of optics from the geometrical as distinguished from the physical point of view, written by one of these gentlemen who is the special expert in that branch. Thus, after the opening chapter on the theory of light, principles of reflexion and refraction, shortest path, the law of Malus, and such things, by Herr Siedentopf, there follows a chapter on the course of rays through optical systems by Herrn König and Rohr. The next two chapters, written respectively by Herrn Wandersleb and Culmann, deal with the formation of images, and here Maxwell's ideal instrument is introduced. Then there follows a most exhaustive and valuable memoir on spherical aberration by Herrn König and Rohr, and another on chromatic aberration. A chapter in which the relation of these two last to the construction of optical instruments is considered follows, and then one by Herr Löwe on prisms, with others by Herr Rohr on the eye and photometry respectively as applied to optical instruments, fill the book. It is difficult to do more than give this catalogue of its contents, but they show that the book is a valuable one and should be at hand for reference wherever telescopes are used.

THE TELESCOPE †.—This is a smaller and less pretentious book than the last mentioned, but on a similar subject. It contains descriptions of each kind of telescope, with very clear diagrams of the courses of the rays through the lens systems. The book was originally written in 1879, apparently as a thesis in a competition for a University Medal, so now, after nearly a quarter of a century, the author has added to the original work chapters on the recent improvements in correcting for achromatism. The invention of the triple objective, of Jena glass, and the investigations of Professor Hastings are described. There is also a section on photographic telescopes and the work done with them, and a bibliography of the whole subject. This little book of 128 pages, which is small enough to be carried in the pocket, can be safely recommended for interesting and useful reading.

NOTES.

COMET NOTES.—Two almost coincident but entirely independent orbits of Brooks' Comet (α 1904) have been published—(1) By

* Edited by M. von Rohr. Julius Springer: Berlin, 1904. Price 18s.

† 'Optical Principles involved in the Construction of Reflecting and Refracting Telescopes.' By Thomas Nolan, M.S., A.M. 2nd edition. New York: D. Van Nostrand Company, 1904. Price 50 cents.

VOL. XXVII.

Prof. Nijland from Utrecht observations on April 17, May 3 and 19; (2) by R. G. Aitken and J. D. Maddrill from Lick observations on April 17, May 8 and 24.

	(1)	(2)
T	1904 Mar. 7 ^h 06 ^m G.M.T.	Mar. 6 ^h 90 ^m G.M.T.
ω	53° 29' 53"	53° 27' 14"
Ω	275 45 59	275 46 5
i	125 7 37	125 7 33
log q ..	0.43248	0.432475

The residuals of the middle Lick place (Observed—Computed) are $\Delta\lambda' \cos \beta' - 1''\cdot 0$, $\Delta\beta' + 1''\cdot 7$; these small residuals show that the orbit cannot differ much from a parabola; it is now quite certain that the objects photographed at Harvard on March 11 and 15 were not the comet, as their places were some 5° in error. Owing to its great perihelion distance, the comet's light will only diminish slowly, and it may be under observation for several months. 'Lick Bulletin,' No. 56, contains the following ephemeris for Greenwich Midnight:—

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ' "		h m s	° ' "
July 2....	12 36 22	52 56	Aug. 11....	12 15 51	46 10
6....	12 31 38	52 9	15....	12 15 59	45 39
10....	12 27 42	51 23	19....	12 16 20	45 11
14....	12 24 29	50 38	23....	12 16 53	44 45
18....	12 21 53	49 54	27....	12 17 37	44 22
22....	12 19 50	49 12	31....	12 18 29	44 0
26....	12 18 17	48 31	Sept. 4....	12 19 29	43 41
30....	12 17 9	47 53	8....	12 20 35	43 24
Aug. 3....	12 16 24	47 17	Nov. 11....	12 39 1	45 6
7....	12 15 59	46 42			

Brightness 0.43, 0.28, 0.21, 0.18 on July 2, Aug. 7, Sept. 8, Nov. 11 respectively; that at discovery being unity.

Ast. Nach. 3957 contains Definitive Orbits of two comets—(1) the Comet 1887 II. (Brooks) (under observation 1887 Jan. 22 to April 23), by C. Stechert; (2) the Comet 1889 IV. (Davidson) (under observation 1889 July 19 to Nov. 21), by Dr. Guido Horn. In each case the elliptic character of the orbit seems established, as the most probable parabola leaves larger residuals than can reasonably be ascribed to errors of observation.

	(1)	(2)
T	1887 Mar. 17 ^h 42 ^m 7 ^s Berlin M.T.	1889 July 19 ^h 32 ^m 30 ^s Berlin M.T.
ω	159° 26' 15"	345° 52' 43"
Ω	279 56 13	286 9 18
i	104 16 10	65 59 11
log q	0.212226	0.016920
e	0.98369	0.99772
Period ...	999 years.	9739 years.

In each case planetary perturbations have been applied, and the elements are osculatory for the time of perihelion passage.

A. C. D. C.

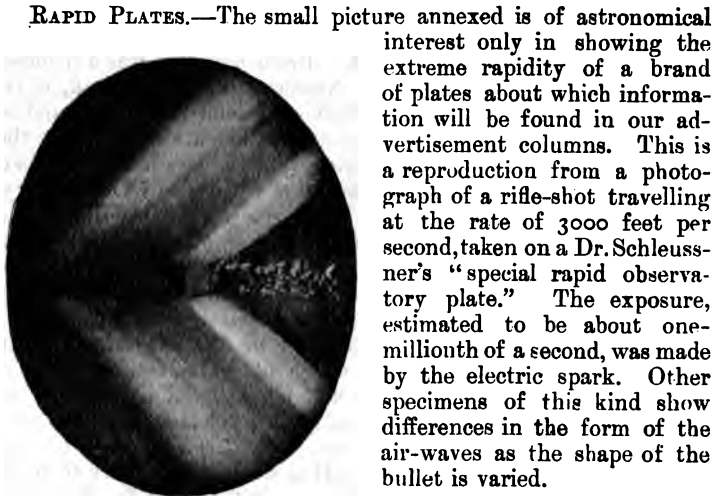
MINOR PLANET NOTES.—Two new planets have been discovered, OE, OF. Details of the former are not yet to hand; the latter was photographically discovered by G. F. Peters at the Naval Observatory, Washington, on May 11, its magnitude being 11.7.

A. C. D. C.

OBITUARY.—THEODORE BREDICHIN. With regret we have to announce the death of this distinguished Russian astronomer, which took place on May 14 last. Bredichin, who was a member of a noble family, was born at Nicolajeff on December 8, 1831. He was first educated at the Richelieu Lyceum in Odessa, and in 1851 became a student in the physico-mathematical faculty of the University of Moscow. In the year 1857 he was made Professor of Astronomy, and in 1873 Director of the University Observatory. He approached astronomy from many sides. He was the first in Russia to make spectroscopic observations of stars. He made observations of star-positions, of time of swing of pendulums, but his most famous works are his valuable contributions to the theory of the forms of comets and of meteor streams. In the year 1891 he succeeded Otto Struve as Director of the Russian National Observatory at Poulkova, from which office he retired in 1894 on account of advancing years and the state of his health, for which the climate of Poulkova was not suitable.

THE FIGURE OF THE EARTH.—It is scarcely necessary to recall the fact that up to the year 1730 it was still a point for discussion as to whether the Earth was an oblate or a prolate spheroid, and that Louis XIV., in order to settle the question, in the year 1735 despatched geodetic expeditions, one to the Equator under Godin, Bouguer, and de la Condamine, the other to the Arctic Circle under Maupertuis, Clairaut being of this party, to measure a degree of latitude at each of these places. And since it resulted that the degree measured by Maupertuis was longer than that measured at the Equator, the oblateness of the figure was proved. It has already been stated in these pages that this classic experiment has been lately, or is now, being repeated. French astronomers and geodesists are in Peru making a triangulation, and a party of Russians and Swedes have been in the far north measuring the length of a meridian arc. The *Bulletin Astronomique* for June contains an account by Prof. Backlund of the experiences of the Russian astronomers, who seem to have met with considerable hardship and difficulties, owing to the ice and the cold, but finally in the years 1900, 1901, completed a triangulation, the base being measured by Dr. Guillaume's method with a non-expansible wire, which gave for the distance between the parallels of latitude $76^{\circ} 37' 44''.6$ and $79^{\circ} 3' 59''.1$ a length of 272,072.3 metres with a probable error of 4.6 metres. The probable errors of the determinations of latitude are given respectively as $0''.2$ and $0''.3$. Observations with pendulums of the Sterneck type were made at five northern stations, as well as at Potsdam, Pulkowa,

and Stockholm. Meteorologists, botanists, and zoologists accompanied the party, and a good deal of information seems to have been acquired. The Swedish geodesists have not yet published their results, and it is believed that the observations at the Equator are still proceeding.



RAPID PLATES.—The small picture annexed is of astronomical interest only in showing the extreme rapidity of a brand of plates about which information will be found in our advertisement columns. This is a reproduction from a photograph of a rifle-shot travelling at the rate of 3000 feet per second, taken on a Dr. Schleussner's "special rapid observatory plate." The exposure, estimated to be about one-millionth of a second, was made by the electric spark. Other specimens of this kind show differences in the form of the air-waves as the shape of the bullet is varied.

THE WEATHER IN JUNE.—The total rainfall for June at Greenwich was less than 1 inch, which is an inch below the average of 50 years, and brings the total amount for the first half of the year to about the average. The month on the whole was cold, as the mean daily temperature for June at Greenwich was about 58°, which is 1½° below the average. The sunshine record is in excess.

ERRATUM.—In last month's number, p. 220, line 20. The focal length of the Greenwich Astrographic Telescope is 11 feet 3 inches; of the Thompson Refractor 22 feet 5 inches.

CAMBRIDGE University has conferred the degree of Doctor of Science on Prof. H. G. V. de Sande Bakhuyzen, of Leyden. The following was the speech delivered by the Public Orator on the occasion:

Inter doctores nostros novus primus hodie progreditur Scientiarum Academiae Amstelodamensis praeses, Batavorum astronomus insignis. Abhinc annos septemdecim consilii magni inter auctores fuit, quo caeli totius stellae, luminis ipsius auxilio chartis impressae, accuratissime redderentur. Etiam altero in opere immenso cum aliis consociatus est, quo caeli parte Boreali in regiones sedecim divisa, stellarum multitudo infinita minutissime observaretur. Juvat hodie recordari caeli regionem astronomo Leidensi assignatam regioni Cantabrigiensis esse conterminam. Idem latitudinis (ut aiunt) varietatem, orbis terrarum axe leviter vacillante exortam, diligenter exploravit. Talium vivorum ope Europae gentes scientiae amore excitatae, etiam in orbe terrarum accuratius dimetiendi invicem certant, astronomi illius antiqui laudem aemulatae,

"descripsit radio totum qui gentibus orbem."

THERE will be an occultation of Aldebaran by the Moon on July 9^d 17^h 31^m—that is, in the morning of Sunday, July 10, civil reckoning. The phenomenon will happen in daylight, but it may prove interesting.

THE Astronomer Royal and Sir David Gill have both been created Corresponding Members of the Paris Bureau des Longitudes.

HIS Royal Highness the Prince of Wales has graciously consented to become Patron of the Royal Meteorological Society.

THE honorary degree of D.C.L. was conferred on Sir David Gill by the University of Oxford on June 22.

FROM AN OXFORD NOTE-BOOK.

OXFORD has just been rejoicing in the installation of its new Chancellor, Lord Goschen. Thirty-seven honorary doctorates were conferred on the occasion, thirteen in science, eight in literature, and sixteen on various people with miscellaneous claims. Astronomy was represented by one name only, that of Sir David Gill, but we cannot find fault with the quality of the representation. The proceedings were necessarily a little long, but the animation of the Chancellor and of one or two undergraduates saved them from being dull. The Chancellor had a new superlative for almost everyone, and seemed thoroughly to enjoy the fun of producing them. The undergraduates did not say anything specially remarkable, now that one thinks of it in cold blood; but those who have to listen to latin speeches for nearly three hours are grateful for almost anything—for a sudden shout of “Hoo-Ray” when Lord Reay was presented, or a groan which rather cruelly imitated the tone in which a tragic prize poem was being recited. Given a proper setting, it is apparently possible to amuse a large audience by saying “Never mind!” in a tone of resignation: the context being, first the reading of a series of rather long latin speeches quite inaudibly, and secondly a number of clearly uttered encouragements to the reader to “Speak louder, Sir!” It was after about the sixth of these remonstrances, and after the ears of the audience had been strained nearly to bursting, that the “Never mind!” brought us all relief.

AT the time of writing I have not been able to recover any of the latin speeches that were made about these distinguished visitors. But about three weeks ago a minor honour, none the less well deserved, was conferred upon an Astronomer here; and I am fortunately able to reproduce the speech of the Public Orator the occasion, *verbatim et literatim* :—

In Observatorio Radcliffiano Francus Arturus Bellamy, quem vobis hodie praesento, per annos XII. tantum non primas partes egit, et suo prope Marte

eaſ praecipue res quae ad meteorologiam ſpectant diligentiffime ordinavit. Inde ad Obſervatorium huius Academiae tranſlatus, eandem praestitit ſedulitatem, ac per XI. annorum ſpatium in operoſis laboribus multos dies multas noctes egit.

Profeſſoris Saviliani adjutor adſiduus magni fuit operis partioeps, unde a cunctis fere nationibus maximae perpetuaeque utilitatis ſunt percipiendae; quippe omnes qui ſcientiam colunt aſtronomica ſtudio communi in hoc incumbunt, ut, totius coeli regionibus accuratiſſime colluſtratis, quotquot ſint ſtellae dinumerentur et luminis ipſius auxilio chartis inprimantur. Quantum ad hanc rem contulerit vir optimus, quantam habilitatem in ſideribus ſervandis adhibuerit ſciunt omnes qui Aſtronomiae cognitionem habent. Porro, ne in unius tantum ſcientiae finibus concludantur ſtudia, idem plenam plantarum, herbarum aliarumque naturae partium notitiam ſibi comparavit, et doctis lucubrationibus litteras adornavit et locupletavit.

SOME interesting reſults bearing on the theory of viſion have been recently obtained in the Oxford Phyſiological laboratory, with the capillary electrometer which Profeſſor Gotch has uſed to ſuch good purpoſe. The exciſed eyeball of a frog has been ſubjected to the influence of various coloured lights, and from the ſummary of reſults I extract the following:—

8. The time-relations of the reſponſe evolved by the various coloured lights and by darkneſs are not identical. The obvious differences in this reſpect are ſo diſtinct as to ſuggeſt four reſponſes independent as regards their Cauſation.

9. The four diſtinct reſponſes are as follows:—

(a) The reſponſe to red light characteriſed by a long latency of nearly 0.3, and by its attaining to a conſiderable maximum, averaging about 0.0004 volt.

(b) The reſponſe to green light characteriſed by the ſame ſhort latency as that found in the reſponſe to white light, *i. e.* leſs than $\frac{1}{10}$ ſecond; it is alſo characteriſed by the magnitude, the maximum reached averaging over 0.0005 volt.

(c) The reſponſe to violet light characteriſed by a latency longer than that of the green reſponſe, but diſtinctly ſhorter than that of the red one ($\frac{1}{10}$ ſecond). It is alſo characteriſed by its low intensity, the maximum reached averaging only 0.00024 volt.

(d) The reſponſe to ſudden darkneſs, characteriſed by a remarkable conſtant latency of not more than $\frac{1}{10}$ ſecond, whatever the character or quality of the previous illumination; this reſponſe is dependent for its production upon the change from previous illumination to the condition of darkneſs and varies in magnitude with the duration and luminosity of the previous light. It is moſt readily obtained if the previous light has been white, is eaſily obtained if this has been green or red, but is only obtained after violet illumination when this has laſted for ſome time, generally 8 ſeconds or more.

10. The reſults appear to be in accordance with the theory of Young-Helmholtz as modified by Maxwell, which aſſumes three diſtinct colour reactions, *viz.* red, green, and violet. In addition they appear to indicate that the eye reacts to ſudden darkneſs.

Beyond the laſt very important inference, the precise bearing of theſe experiments on, ſay, aſtronomical obſervations is of courſe ſtill very remote and obſcure; but there may come a time when we ſhall find the phyſiologiſts helping us more directly.

A COUPLE of months ago, in a note or two of welcome to Newcomb's 'Reminiſcences of an Aſtronomer,' I regiſtered a reſolve to get hold of a copy of the article by Mr. John Fiſke on "Eccentric

Literature" in the *Atlantic Monthly*. My excellent bookseller has now produced it (the date, not given by Newcomb, is March 1899) and I have been reading it with joy. There is not very much new astronomical paradox — De Morgan's classical 'Budget' is still drawn upon for many of the illustrations in that department,—but sufficient amusement is provided in other directions. The article opens with a graphic account of the formation of the section of "Eccentric Literature" in the Harvard University library, where Mr. Fiske was assistant librarian "about five-and-twenty years ago." It distressed him to find circle-squaring literature classified under mathematics, and crazy interpretations of the book of Daniel under Biblical Criticism; and he collected all such things together and labelled them "Insane Literature." But one of his assistants came to him in distress to point out that some of the writers of these pamphlets were in the habit of using the library and might not like the new classification; and to spare their feelings the epithet "Insane" was altered to "Eccentric," which Mr. Fiske flattered himself would be considered by those most concerned rather complimentary than otherwise. "Thus the class Eccentric Literature was established in our catalogue, and there it has remained, while the books in the library have increased from a hundred thousand to half a million. Once or twice, I am told, has some disgusted author uttered a protest, but the quiet of Gore Hall has not been disturbed thereby."

ONE of the Daniel-Apocalypse eccentrics juggles with two periods of 1082 years, which he breaks up "(Heaven knows why!) each into four periods of 68, 204, 269, and 541 years. Then we are treated to the following equations:—

$$\begin{aligned} 68 &= 2 \times 34 \\ 204 &= 6 \times 34 \\ 269 &= 5 \times 34 + 3 \times 33 \\ 541 &= 13 \times 34 + 3 \times 33 \end{aligned}$$

Hence, 'with such a fulcrum as the Lamb slain before the foundation of the world, and such a lever as the span of the Victim's sublunary humiliation, was I too rash in arriving at a result infinitely grander than Archimedes's speculative displacement of the Earth?'

MR. JOHN FISKE truly remarks that it is most difficult to draw the line separating eccentrics from enthusiasts. He is himself most cautious, and writes as follows:—

Even the early circle-squarers cannot fairly be charged with crankery; they sinned against no light that was accessible to them. But anybody who today should advertise a recipe for turning base metals into gold would meet with a chill welcome from chemists. He would speedily be posted as a quack, though doubtless many weak heads would be turned by him.

It is not yet five years since these words were written, and we have already become accustomed to the idea that one element may possibly be turned into another—radium into helium, for instance.

The fact may not yet be fully established, but we have become accustomed to the idea. Five years hence, shall we be so sure that the alchemists were misguided in searching for the method of turning base metals into gold?

RETURNING to Prof. Newcomb's book, which I have to thank for guiding me to Mr. Fiske's article (see p. 382), there is, on the preceding page (p. 381), an allusion to some work of Peters, which gives, in my opinion, a very unfair view of the action of Mr. Knobel. I have some knowledge of the circumstances, and hope that Prof. Newcomb will allow me to say a word or two in correction of what I cannot but regard as an injustice done. It is stated that Mr. Knobel claimed, as a collaborator with Peters, the papers representing "a critical examination and comparison of all the MSS. of the *Almagest* in the libraries of Europe, or elsewhere, whether in Arabic or other languages," and that during the twelve years that have since elapsed, nothing more has been heard of the work." Moreover, it is added that "No one, so far as I know, ever heard of Peters's making any allusion to Mr. Knobel or any other collaborator." The language is carefully guarded, but the impressions suggested are only too obvious; and it is these impressions or implications which I have no hesitation in pronouncing unjust. In the first place, there was an undoubted and perfectly definite arrangement as to the partition of the work: Mr. Knobel was to search the English libraries, which Peters had never touched. Secondly, as regards what has been done since, Mr. Knobel has spent the greater part of his scanty leisure, and many pounds sterling, in deciphering and arranging the crabbed MSS. of Peters, written on all sorts of scraps of paper in the most illegible German handwriting. That nothing has yet appeared in print on the subject is due to the quantity and difficulty of the work, and not to inertia.

DEAR MR. OXFORD NOTE-BOOK,—

It is curious how frequently our entertaining writers get their Moons wrong. The more so as it does not require any recondite astronomic lore to avoid most of the errors committed. Common observation and an average memory ought to suffice to remind a writer, even in the recesses of his study, that we cannot very well have a waning moon on view at seven o'clock in the evening.

Yet in the following passage from Algernon Gissing's '*Knitters in the Sun*,' chap. xii. p. 140, a waning Moon seems to obey artistic rather than natural laws:—

"It had been the first mild day of March, a wonderful spring day of absolute stillness and thunder-like warmth, with gleams of sun from amid huge sombre clouds with magnificent crests. There were now patches of stars, with a faint light from a waning Moon."

Yours faithfully,

W. T. R.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXVII.

AUGUST, 1904.

No. 347.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1904 June 29.

President: S. A. SAUNDER, M.A., in the Chair.

Secretary: A. C. D. CROMMELIN, B.A.

THE Minutes of the two previous Meetings were read and confirmed.

Mr. Crommelin read the list of presents received, and the thanks of the Association were accorded to the respective donors.

The names of four Candidates for Membership were read and passed for suspension.

On the motion of *Mr. E. Holmes*, seconded by *Mr. W. London*, Messrs. Gordon Miller and F. D. C. Baly were appointed auditors of the Session's accounts.

The President appointed Messrs. A. J. S. Adams, George Bruford, and D. W. Horner as Scrutineers of the ballot for the new Council. The list prepared by the Council for the purposes of the ballot was then read and no additions being made was declared closed.

Mr. Crommelin gave some extracts from a paper by *Mr. W. F. Denning* on "Taurid Meteor Showers."

Mr. G. F. Chambers said he was sure that the members of the Association would be sorry to hear that *Mr. Denning* was, and had been for several months, very unwell and unable to carry on his valuable meteoric observations.

The President expressed the thanks of the Meeting for the paper and hoped they might soon hear of *Mr. Denning* being in better health.

Col. Markwick read the Interim Report of the Variable Star Section and concluded by reading a communication from Prof. Moye,

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2 D

of Montpellier, informing him that their local astronomical society had taken up variable star observation and would be glad to work in conjunction with the Section.

In reply to a question by *Mr. Hardcastle, Col. Markwick* said that by position-angle he meant the apparent position of the variable star with regard to the comparison star. The variable star might be at one time vertically above and at another vertically below the comparison star. That would apply particularly to circumpolar stars. It appeared that there was some physiological tendency to record the star as brighter when below the comparison star than was really the case.

The President remarked that "position-angle" was rather a misleading term to use for what *Col. Markwick* had been describing. What was technically known as position-angle between two stars never altered.

Col. Markwick said that what he meant was the angle made by the line joining the two stars with the vertical passing through one of them.

Mr. Crommelin read a post-card from *Prof. Meye* strongly supporting the advisability of eclipse parties going to the Mediterranean coast, and suggesting the little town of Castillon de la Plana, about 100 miles south of Barcelona, as very suitable.

Mr. Chambers said there were, of course, two ways of getting to Spain from England for the purpose of observing the eclipse—overland by France, or by sea, landing either on the north or east coast of Spain. It seemed to him that they had the prospect of a very successful and pleasant expedition, if a sufficient number of persons would come forward and offer to take part in it, in the sea expedition from England to the coast of Spain—that was to say, chartering a steamer to land people on the north coast who might wish to land there and then proceeding to the Mediterranean coast with those who might desire an extended sea voyage. But such arrangements, to be satisfactorily made, took time, and it was by no means too early for preliminary preparations to be made.

Lord Hampton asked what time would be occupied if the trip were made wholly by sea.

Mr. Chambers thought it would be a three weeks' trip.

Mr. Crommelin said that for those who were afraid of the Bay of Biscay there was the route by rail to Marseilles and then by steamer down the Spanish coast.

Mr. C. T. Whitmell read a paper on "The Visibility of the Satellites of Jupiter with the Naked Eye." The writer stated that while naked-eye observations of Jupiter's Satellites were certainly rare, so rare that many persons entirely rejected and others gravely doubted such evidence as was available, there was evidence, which could not well be put aside, that the satellites were occasionally visible to the naked eye. The paper then proceeded

to discuss in detail a number of the best authenticated instances on record.

Mr. Thwaites quoted the case of a missionary in Asia Minor who constantly saw the Satellites of Jupiter and added that Arabs had often seen them.

Mr. A. J. S. Adams spoke, from personal experience, of the wonderfully clear atmosphere of the highlands of Asia and was not surprised that some of Jupiter's satellites could be seen there without much difficulty.

Mr. Chambers said that he had had occasion in the past to go very carefully through the various statements which were available for throwing light upon the question, and he was bound to say he had come to the conclusion that it was impossible not to believe the statements made that the satellites had been seen by the persons who said they had done so.

Mr. E. Holmes said it was time they had done with the story of that missionary in Persia, since he saw the satellites of Jupiter night after night at the same hour. He did not think any supposed observation of a satellite of Jupiter was worth notice unless the date and hour were given.

Mr. Chambers said the absence of dates did not affect the credibility of these observations. Only trained astronomers were precise in recording dates.

Mr. Crommelin said there was one point which made the date of importance. It was quite possible for an observer to mistake a small star for a satellite, and this could not be verified in the absence of the exact date.

Mr. Hardcastle referred to a story he had heard that the satellites could be seen in a mirror when they could not be seen directly.

Mr. Crommelin thought the explanation of that was pretty obvious. What was really seen was a series of images of rapidly diminishing brightness through successive reflection between the two surfaces of the mirror. There would be a row of faint images of the planet mistaken for satellites.

Mr. Whitmell said he had himself shown pretended satellites in that way. He had brought the paper forward because it seemed to him that the evidence for the matter had not received quite fair treatment, and there was sufficient evidence to make him hesitate before denying the thing altogether, as many people were inclined to do. He had always felt that a definite, positive case outweighed a thousand negative cases, and he did not see any way of getting over some of the cases he had mentioned.

Mr. Whitmell next read a note on "Mock Suns." A note by *Mr. W. H. S. Monck* on the "Sun's Motion in Space" was also read. This being the last meeting of the Session, the Meeting stood adjourned till the last Wednesday in October.

Captain Noble.

THERE was more than one member of the Royal Astronomical Society to whom this name and style applied, but it was sufficiently distinctive to most of the Fellows who attend the Meetings. They called to mind but one personality—a long, lean figure which met one with a warm hand-shake, and followed up the greeting with a whisper of his latest good story or the exact humorous quotation applicable to the occasion. This was Captain William Noble, F.R.A.S., whose loss many astronomers, amateur and professional, now have reason to deplore.

He was not the Senior Fellow of the Society, but he was among the first ten, and had probably attended more Meetings than any other. He was elected in 1855 June, and very shortly after began to take part in the proceedings of the Society, for an account of an occultation of Mars communicated by him will be found in the 'Monthly Notices' for 1857, and there are few volumes among those for many years after which do not contain some contribution from his pen. He was an excellent observer of such small phenomena, inasmuch as he set down what he saw, and exactly what he saw, however apparently improbable and trivial it might seem at the time. There will be found, for instance, in the 'Monthly Notices,' a note by him to the effect that when observing the Sun one day in a hazy sky, the focal length of the object-glass apparently shortened by 0.11 inch, but when the haze cleared, the focus returned to its normal position—a remark which may not have had any immediate bearing, but was simply the expression of a well-observed fact, which might find an explanation hereafter. It is not quite certain when he began to interject in the discussions at the Meetings those remarks which were so characteristic of him, for in the earliest years of his fellowship verbatim reports of the proceedings were not made; but at the Meeting on 1864, April 8, there was a discussion about the willow-leaf markings on the solar surface (a not unusual subject in those days), and the account relates that Capt. Noble said that once he thought he saw them, and "if I had been Archimedes or a patent-shirt maker, I should have cried 'Eureka!'"—which is just the kind of remark that he continued to make until the end of his life. No discussion was so serious but that Capt. Noble could get up and make some little speech, possibly wholly irrelevant, but always amusing, and for this, if for no other reason, he will be missed at Burlington House.

But his remarks were not always irrelevant. Capt. Noble was fearlessly outspoken on matters that he thought called for remedy, even to the verge of prejudice. Delayed publication of the 'Monthly Notices' frequently induced him to make pertinent enquiry of the President. The supposed shortcomings of the *Nautical Almanac* under a former regime often led to his sarcastic

comment, but his *bête noire* was the Solar Physics Committee and its work. One wondered why he was so bitter on the subject, for he was a man of discernment and could appreciate good work, even though it was not strictly utilitarian; but the fact seems to have been that about the year 1880, when the proposal was brought forward for the establishment of an observatory at South Kensington at the public expense, some popular opposition arose, and Capt. Noble was the Secretary of the Society for Opposing the Endowment of Research. The observatory was established, as every one knows, and no doubt the Society was disbanded, but its Secretary, being a consistent man, continued his opposition. Each fortnight, for many years past, a long letter has appeared in the *English Mechanic* over the signature "A Fellow of the Royal Astronomical Society," which it is an open secret was a pseudonym for Capt. Noble, often containing criticisms on the work of the Kensington Science Department, almost libellous, but those who knew the writer were aware that his "bark was worse than his bite."

Besides observations of the kind indicated above, occultations of stars and planets, of sun-spots, and other occasional phenomena which he communicated to the Society, he made, but has not published, an excellent series of drawings of the surface of Jupiter, and his fortnightly letter to the paper above mentioned showed how completely he kept himself informed of all that was going on in the science. In this letter he played the part of mentor to the many amateurs who are correspondents of the journal, and no doubt this, combined with his genial personality, caused him to be selected as the first President of the British Astronomical Association. He contributed sometimes to these pages, and in the early days of *Knowledge* wrote for that magazine a series of papers called "Nights with a 3-inch Telescope," which were afterwards published as a book. But astronomy was not his chief occupation. He lived the life of a country gentleman at Maresfield, in Sussex, where he was one of the best-known and best-liked men in the neighbourhood, and filled many of the parochial and public offices justly and zealously. For 46 years he sat as Magistrate at Uckfield Petty Sessions, and was Chairman of the Bench at the time of his death. For 34 years he presided over the meetings of the Uckfield Board of Guardians, and was this year elected Chairman for the thirty-fifth time. He had a special distaste for tramps, against whom he waged bitter war; and if one were brought before him he awarded, if possible, a sentence involving stone-breaking, for, said he, "it spoils the delicacy of their hands for picking pockets, which is their normal occupation"—a sensible point of view which was quite characteristic.

As to actual details of his life, Capt. Noble was the eldest son of the late Mr. William Noble, of Berwick, and was born in London in 1828. He was educated privately, and served for a

time in the Rifle Brigade, where he reached the rank always associated with his name. In 1851 he married Emily Charlotte, the only child of Mr. Edward Irving, and had four sons; one, who survives, has lived abroad for many years, the others predeceased their father, the youngest leaving a widow and two daughters. Mrs. Noble died a few years ago. Capt. Noble was elected to the Council of the Royal Astronomical Society in 1866, and sat at the board almost continuously after that date. It may be mentioned, as illustrative of one phase of his character, that he refused for many years to be a member of the R. A. S. Club, although invited to do so, for he seemed to think that this was a clique inimical to the best interests of the Society, but quite lately withdrew his opposition and joined the circle. The immediate cause of his death will be gathered from the following extract from a letter received by the writer of this notice from him on April 18 last:—

. . . I have been somewhat seriously ill. At the beginning of the year, thanks to our infernal climate, I got a chill, which brought on kidney and bladder troubles, and I am still under medical (and surgical) treatment. My chief present grievance is my inability to eat, and I am as weak as a rat. I am ordinarily a great walker, but a mile is quite as much as, or more than, I can manage at present. I do hope that I shall pick up enough to be present at the May meeting of the R. A. S., but, like the man you may have heard me quote before who went for four ounces of mixed tea, "I hope for the best and am prepared for the worst."

He did not appear at the May meeting, and grew rapidly worse during May and June, suffering a good deal of pain at the end, but bore up in the most wonderful way, and tried to keep up all his pursuits till the last. The end came on Saturday, July 9.

H. P. H.

Dr. Isaac Roberts, F.R.S.

ISAAC ROBERTS was born at Groes, near Denbigh, January 27, 1829; but whilst quite young his parents settled in Liverpool, where he received a merely elementary education. He was apprenticed at the age of 14 to a builder and contractor, and mastered the details of his business so thoroughly that he afterwards became a consulting expert in all matters relating to the craft. He worked usually 13 hours a day, yet found time to join the evening classes that were then available, and laid the foundation of that taste for science which was in after years to make him famous. Being very successful in business, he amassed a considerable fortune, which enabled him to indulge in scientific pursuits—though still retaining a nominal interest in a great contracting firm. Geology was at first his favourite subject, and he became a Fellow of the Geological Society at an early date; but he also paid considerable attention to chemistry, electricity, microscopy, spectrum analysis, &c., as the numerous instruments and apparatus stored away at

Starfield abundantly testify. He read a valuable paper before the British Association in 1878 on the filtration of sea-water through triassic sandstone; and also carried out a series of elaborate experiments from 1882 to 1889 on the movement of underground water in the vicinity of Liverpool as affected by barometric and lunar changes. He read numerous papers on geological and kindred subjects before the local societies, where he was looked upon as quite a representative citizen. By means of specially designed weighing-machines he carried out exhaustive experiments on the vertical and lateral pressures of various kinds of grain, sand, gravel, shot, &c., when stored in cells up to 80 feet in height (as required in actual practice at the docks); some of these results are published in the Royal Society's 'Proceedings.' On leaving Liverpool for Crowborough in 1890 he was presented with an address signed by many scientific men connected with University College and other local bodies. His first beginnings in Astronomy date from the time he lived at Rock Ferry, Birkenhead, where he had a .7-inch Cooke refractor in the late seventies. Afterwards, at Maghull, he turned his thoughts seriously towards celestial photography, ordering a 20-inch reflector from Sir Howard Grubb, to be mounted on the same equatorial stand as the refractor—the clock-movement being common to both, but each moving independently in declination. This, then, after a year spent in altering and perfecting details, was the original equipment with which Dr. Roberts commenced, in 1885, his photographic chart of the northern hemisphere—and is practically the same as that used up to the day of his death, the chief alteration being the substitution of a Calver mirror for the Grubb in 1888, and the addition of a 5-inch star-camera by Cooke in 1895. Then came the epoch-making star-pictures by the Henry Brothers, and the subsequent Photographic Convention at Paris (which Dr. Roberts attended), when the International scheme for charting the whole heavens was elaborated. Seeing that this work was planned on so imposing a scale, Dr. Roberts wisely turned his attention elsewhere, and in fixing upon the photography of nebulae and star-clusters, he showed singular prescience as to the peculiar fitness of the reflector for such objects. Even at this time he recognized that the gelatine film of the modern dry plate was a perishable product, and actually spent some hundreds of pounds on an elaborate instrument for engraving directly upon copper plates the stars on the glass negatives—thus leaving a practically imperishable record. Unfortunately, however suitable this might be for stars, it was utterly useless for delineating the nebulae, and so fell into abeyance, except as a measuring-machine. This instrument, which he designed himself and called the "Pantograver," was made by the late Adam Hilger, and is unique of its kind. Very few years' experience convinced him that the climate of Maghull was not suitable for delicate photographic work; so, after much diligent enquiry, he decided upon

the summit of Crowborough Hill, Sussex, some 800 feet above sea-level, as the apparently ideal spot for an observatory. Here, then, the Starfield Observatory was erected in 1890, the same year in which he was elected to the Fellowship of the Royal Society; and the chief work carried on there ever since has been the photography of star-clusters and nebulae—thousands of negatives being stored as the result of this persistent labour.

In 1892 the degree of D.Sc. was conferred upon him by Trinity College, Dublin, on the occasion of its tercentenary. And in 1895 he received the Gold Medal of the Royal Astronomical Society (of which he had been a Fellow since 1882), in recognition of his unique and valuable contributions to astronomical science.

Two handsome quarto volumes of 'Photographs of Stars, Star-clusters, and Nebulae,' published by him, testify to the good work done during this period, Vol. I. being issued in 1893 and Vol. II. in 1899. Besides these, quite a respectable supplement of celestial photographs has appeared in *Knowledge* at various dates; also many papers in the 'Monthly Notices' of the R.A.S. The last literary work in hand was a kind of general Index of all his published papers, with a short *précis* of each; this is left in MS. at present.

Dr. Roberts was a man possessing considerable force of character, with a great capacity for work, and an indomitable perseverance that surmounted all obstacles. He would frequently be at work in his library by 6 o'clock in the morning; and on the last day of his life he was up quite early and busily engaged in examining negatives, up to within an hour of the time when he was stricken down with such tragic suddenness. Less than a week before he had a sunshine recorder fixed up, with a view to comparing the relative amount of sun in England and the Riviera, where he anticipated spending the winter months. As a mechanic he had talents of no mean order, and possessed a fertility of resource in devising new experiments that many would be proud of. He would elaborate a scheme on paper, leaving only constructive details to be worked out; and it was seldom that the result failed to answer the anticipations. As might be expected, his tenacity of purpose and firm adherence to his own views sometimes brought him enemies, but he was simply influenced by an intense desire to get at the truth, and cared nothing for personalities. As a friend he was generous to a fault, and those who knew him best appreciated him the most. He was one of the earliest and most consistent advocates of the merits of the reflector for celestial photography, and lived to see his predilection confirmed in quarters where there had previously been a strong prejudice for refractors. His views on the relative performance of camera-lenses are well known; but it is not so well known that he had a very perfect star-camera fixed on the tube of the 20-inch reflector, with which all objects were photographed in duplicate during the last 9 years; and it was the unvarying superiority of

the reflector plates that made him so sceptical as to much that was called nebosity on camera plates by other observers.

The immediate cause of Dr. Roberts's sudden death (on July 17), as revealed by the *post-mortem* examination, was heart failure, accelerated by the heat of the weather ($84\frac{1}{2}^{\circ}$ in the shade). He leaves no family. He was twice married, the last time in 1901, to Miss Dorothea Klumpke, D.Sc., of the Paris Observatory, a lady well known in astronomical circles.

W. S. F.

Nebulous Double Stars.

At last we seem to be fairly confronted with the pregnant question whether nebulous matter can effectively impede motion. It has long been hovering on the verge of astronomical consciousness. Stars plunged in diffuse nebulosities could not be supposed stuck fast like fruits in a jelly; but their velocities, and the changes possibly impressed upon them, remained conjectural. Then Sir John Herschel described several composite stars as occupying nuclear positions in small nebulae, unmistakably their inalienable appendages; and Mr. Burnham discovered in 1891 an exquisite pair, just divisible with the Lick 36-inch refractor, planted at the exact centre of a large round nebula in Monoceros (N. G. C. 2182). It has not been re-measured; but the odds are "beyond arithmetic" that it is binary, and that its revolutions are conducted in non-vacuous space. Nor could it escape notice that the same conclusion applied to stellar groups apparently fixed, yet certainly physical, and nebulously involved, such as the famous Trapezium and its analogue in the Trifid nebula. The perplexity, however, was, in a manner, latent; it has been rendered active and insistent by certain recent spectroscopic results.

Chiefly through the researches of Professors Frost and Adams with the Bruce spectrograph, into the radial velocities of stars of helium type, we have been made acquainted with at least seven swiftly circulating pairs, the actual envelopment of which in nebosity is scarcely open to question. We have, accordingly, to choose between two alternative views. Either such systems are not destined for permanence, the canker of a resisting medium lying at their root; or matter can exist in the state of a frictionless fluid. The seemingly unopposed flights of comets through the solar corona lend some countenance to the latter supposition, subversive though its implications be.

The detection of the chief component of the Orion Trapezium as a spectroscopic binary was virtually made by Sir William and Lady Huggins in 1897; it was fully ratified by Professors Frost and Adams in 1904. The radial speed of this star varies to the extent of over 60 kilometres a second, the dark, diffuse stellar lines shifting to and fro with reference to the thin bright nebular rays projected upon them. The star is not then in front of the

nebula, as might once have been thought possible; it is within its surging folds. The second θ Orionis (Bond 685), which crosses the meridian six seconds later than θ_1 , is similarly coupled with a sensibly obscure attendant body. And in its case the changes of velocity exceed 140 kilometres a second.

The addition of ι Orionis to the already long list of spectroscopic binaries is of particular interest. In its nebulous capacity, the object figures as N. G. C. 1980. Sir John Herschel recorded it to be "involved in a feeble nebula 3' in diameter"; and Professor W. H. Pickering's photographs depict its connexion, by a glimmering band, with the Trapezium formation. It is also registered as a triple star (Σ 752), consisting of a 3.2 and a 7.3 magnitude component at 11", with a much fainter satellite (called "grape-red" by Admiral Smyth) at 49". Its spectral lines, among which Mr. McClean identified some due to oxygen, oscillate, moreover, with an amplitude corresponding to changes of speed up to 70 kilometres a second. Further intricacies are suspected. On most of the plates taken, the Yerkes investigators tell us*, "the helium lines and $H\gamma$ consist of exceedingly broad and diffuse lines, upon which are superposed maxima sometimes to the number of two or three, and often of considerable intensity. The effect of these (we can readily believe it) is greatly to complicate determinations of velocity." Whether these multiple rays emanate from additional members of the system, or indicate turbulent processes in a single luminous body, cannot be promptly decided. Nor can we hazard a conjecture as to the manner in which its nebulous surroundings are concerned—if concerned at all—in the production of these phenomena.

Most of our readers still vividly recall the surprise which greeted the announcement, in 1885, that Maia in the Pleiades appeared on the Henry plates of four hours' exposure to be adorned with a nebulous appendage in the shape of a squirrel's tail. Professor Adams now enrols the star among close binaries†. Its opposite velocities, it is true, are not large, but they are well-assured; since the shifting lines that indicate them are, under the circumstances, of unusual definiteness. They, indeed, present so strong a dissimilarity to the hazy bands characterizing most nebulous stars, that the Yerkes enquirer is driven to suggest a merely optical relationship between the great white orb and its dim, curving train. Much more convincing evidence, however, than is at present available must be alleged before the proposed severance need be taken into serious consideration.

S (15) Monocerotis is nominally a variable. Winnecke observed it to fluctuate, in 1867, from 4.9 to 5.4 magnitude in a period of $3^d 10^h 38^m$; but its light-changes were either illusory or transient. The object, nevertheless, can put forward other claims to distinction. It is the leader of a small cluster (N. G. C. 2264) in the Milky

* *Astrophysical Journal*, vol. xviii. p. 386.

† *Ibid.* vol. xix. p. 341.

Way, 44 members of which have been measured by Bruno Peter with the Leipzig heliometer. With two fixed companions at $2''\cdot8$ and $16''\cdot6$ respectively, it constitutes the triple object $\Sigma 950$; and to these is now added an invisible satellite causing its recession to vary in rate from 11 to 36 kilometres per second*. The most striking peculiarity of S Monocerotis has, however, still to be mentioned. On one of Professor Barnard's "Willard lens" photographs, taken February 1, 1894, a great encompassing nebula, some three degrees in diameter, imprinted itself†; and although the star is not located at the focus of its strongest condensation, it obviously belongs to the structure. It is, moreover, individually nebulous. Dr. Peter perceived the ex-variable to possess a separate hazy envelope, which could not be expected to become manifest photographically. The conclusion that, in this case too, orbital circulation is carried on in the midst of a nebula is enforced by spectroscopic analogies. The star belongs to Miss Maury's "Group 1," with which she starts the race of stellar evolution. Diffuse absorption by helium and cosmic hydrogen are its distinctive features, oxygen lines being also commonly recognizable. Stars yielding this quality of light are so often nebulous that the presumption is almost irresistible of the association being genuine; and thus the spectral stamp of S Monocerotis affords satisfactory assurance that it is, in fact as well as in appearance, immersed in light-mist.

In Dr. Max Wolf's photographs of the "America" nebula in Cygnus, the $4\cdot6$ magnitude star 57 Cygni was noted to be markedly nebulous‡. It gives a helium spectrum, the lines in which shift, Professor Frost discovered in 1903, to an extent indicating variations of more than 90 kilometres a second in its movement of recession. And a range of 50 kilometres has since been determined for changes in the velocity of σ Scorpii by Mr. Slipher, of the Lowell Observatory§. This star was described by Professor Barnard in 1897|| as forming the nucleus of a conspicuously dense region of the great nebulous field centred on ρ Ophiuchi. Prongs of inchoate stuff extend far to the north and south-west of the spectroscopic binary, and may be inferred to issue more or less immediately from it.

Seven star-couples are thus so far known to circulate rapidly in a nebulous medium; the circumstances of their revolutions have, however, still to be ascertained; and the critical details will need careful and unprejudiced consideration. The supposition is admissible that the widening of their orbits through tidal friction might, for a long time, serve to neutralize the accelerative effects of resistance, if resistance to their motion be indeed offered. But the equilibrium would not be permanent; the momentum of the

* Frost and Adams, *Astroph. Journ.* vol. xix. p. 154.

† *Knowledge*, vol. xix. p. 109.

‡ *Knowledge*, vol. xxv. p. 225.

§ *Bulletin*, No. 11.

|| *Popular Astronomy*, Sept. 1897, pp. 229, 232.

system would be subject to a two-fold waste; and eventual collapse should ensue. Only its postponement could, in this way, be brought about. The alternative hypothesis that nebulous matter does not, in any degree, check motion, though strange to our experience and bewildering to our conceptions, should not therefore be peremptorily rejected. Strictly terrestrial ideas of what is possible inevitably widen in scope as we search the skies. "Look around you, my Lord," his friends warned Francis Bacon when things went badly in his household. "I look above me," replied the great "Chancellor of England and of Nature."

AGNES M. CLERKE.

Ancient and Modern Ideas about the Milky Way.

[Continued from p. 274.]

THE great German philosopher Emmanuel Kant published a *Theory of the Sky* in 1755, in which he affirmed that the proper motions of the stars up to the extreme limits of the Milky Way are caused by mutual attraction. Everything, according to him, gravitates around a central star, which might be the brilliant Sirius. Now, does the Milky Way include the whole Universe? Kant did not think so. There were already known at that time some globular or elliptical nebulae. Each of them, according to the German philosopher, is another Milky Way separated from the first by incalculable distance and all describe majestic orbits around the same central unknown body.

With the *Cosmologic Letters* of the mathematician Lambert, which appeared in 1761, a reaction set in. Lambert claimed a real individuality for the group of brilliant stars of which the Sun forms one. If we are included in the Milky Way, said he, we should not be able to see its limits any more than we can draw the contour of a cloud in which we are enveloped. Let us consider the Sun and the planets as a system of the first order; all the bright stars accompanied in the same way by planets form a system of the second order. Grouping a sufficient number of such systems and we have a Milky Way, a system of the third order. The Milky Way becomes an assemblage of distinct stellar hosts arranged in a ring, as the first glance seems to show, or in the interior of a flattened disc according to the conception of Thomas Wright. There is no reason why there should not be groups of Galaxies which would compose systems of the fourth order, and every great nebula, such as the one we see in Orion, represents perhaps one of these systems seen from outside. For all the systems on each scale, as well for the bright stars as for the Galaxies, Lambert postulated the existence of a central star, a preponderant attracting body, which was necessary in his view to ensure the unity and stability of the system.

Of these ingenious hypotheses the greater part still wait for experimental confirmation. Neither Lambert nor Kant, who were theorists rather than observers, was able to supply such; but with William Herschel observation had first place. No one had gone beyond him in the art of interrogating *Nature* with patience and sagacity, always guided by a fixed plan, but at the same giving equal weight to the facts which supported and to the facts which controverted his theories. In the troubled times which extended from 1780 to 1818 the *Philosophical Transactions* contained not less than seventy-three memoirs from Herschel on the subject now before us, and he surely might say at the end of his career: "The knowledge of the structure of the heavens has always been the final end of my researches."

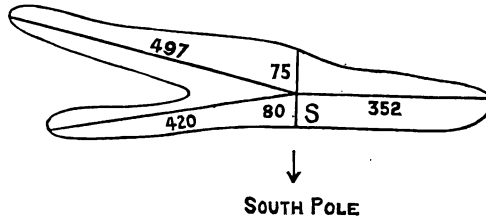
In his earliest works William Herschel adopted without reserve the point of view of Thomas Wright; and if he renounced it later, it was only in such particulars as was necessary to keep it in accord with facts. But he had a great advantage over his predecessor. The telescope which he used, constructed by his own hands, was of a power unequalled at the earlier time. It resolved effectively the Milky Way—that is, it showed its most brilliant parts to be composed of well-separated bright points strewn on a black ground. Herschel therefore concluded that the nebulae which remained irresolvable are systems much further away, situated outside the stellar world properly so called. This view is quite within the scope of our researches and we can sound these extreme depths.

To express these depths in numbers, Herschel took as a sufficiently definite unit the mean distance of stars of the first magnitude. He assumed that the light of a star varied in the inverse ratio of the square of the distance. The proportion gave him in terms of the chosen unit the mean distance of stars of any magnitude; for example, that of the faintest stars visible in his telescope. We do not know how the density of stars varies from one region to another in space. The most simple plan is to consider the density as uniform. If this is so, the number of stars visible in the field depends only on the depth of the stellar universe in the direction in which one observes, and simple geometry shows that the number varies as the cube of the depth.

If this supposition may be made at least as an approximation to the truth, the sounding of the sky becomes a question of elementary arithmetic. The telescope is directed so as to make a continually increasing angle with the plane of the Milky Way and the number of stars seen at each pointing is counted. The decrease is quite marked, especially rapid at the beginning and for the small stars. When one comes to 90° from the initial plane the very faint stars have become rare, and the opinion of Herschel is that a telescope capable of showing stars of the tenth magnitude suffices to penetrate in this direction up to the real limits of the system.

To analyse the entire sky in this way, it would be necessary to

make 833,000 operations, a task beyond the powers of a single observer. Herschel contented himself with 3,400. The doubling of the Milky Way for about a third of its apparent length argues an irregular form externally. Herschel explains this by supposing that the universe of stars does not form a simple flattened disc, but that its thickness is divided in one part like an open book of which some leaves had been raised. The annexed diagram shows the section



made by a plane which would contain the Sun and the poles of the Milky Way. We should be placed, as may be seen, not very far from the centre, just a trifle towards the northern limit. The unit of length is, as has been already said, the mean distance of stars of the first magnitude—a journey that light travelling at the rate of 300,000 kilometres per second would take at least 15 or 20 years to accomplish.

This theory was received by the astronomers of the time with much favour. To-day classic works still quote this as the definitive idea of Herschel. The truth is that its illustrious author, without waiting for the objections which could be made to him, spontaneously retracted two essential points, but even then the accessory part of his work has not escaped criticism.

First of all, have we the right to assume, as Herschel first assumed, that the distribution of stars in space is uniform? There would not seem any great difficulty in supposing this so long as one uses a powerful telescope not covering a very large field, but when one takes a small telescope, or when a celestial photograph taken with a long exposure and a short focus is examined, the impression is given of a structure capricious and complicated. Regions relatively blank border on fields of extreme richness. Groups of stars as well as nebulae present branchings, divisions, and cuttings similar to those that we remarked in the Milky Way. By degrees Herschel was led to recognize, among the stars near to one another, physical groups, conglomerations real and not merely fortuitous. He expressly declared in his later writings that he had modified his ideas, that the stars form separate swarms separated one from the other by large spaces, and that one has no right to consider the richness of the field as measuring the extension of the sidereal universe in a given direction.

In the second place, are our optical instruments capable of

seeing the star farthest away? Herschel thought so in 1786. Thirty years later he did not think so. He found regions more and more in number where the background of the sky appeared nebulous but refused to dissolve itself into stars, and consequently remaining unsoundable by his telescope. In these directions we cannot say what is the depth of the sidereal world, but we know, however, that it is limited; for, if it was infinite, the rays of all the stars which are not hidden by others would be added together so as to give the background of the sky a luminosity equal to that of the Sun. But we know that this is certainly not the case.

This limitation was not considered by Wilhelm Struve sufficiently proved. In his 'Studies of Stellar Astronomy' (1847), the great astronomer of Poulkova thought that the limits of the starry sky are inaccessible not only in some exceptional directions, but all in the plane of the Milky Way. If the light of the stars very far away is not appreciable to us, it is because their light submits to a depreciation on the journey, contrary to what one has supposed. It is thus explained why a certain theoretical increase of power of the telescope does not yield an equivalent increase in the number of stars visible. Combining this remark with the hypothesis of a uniform distribution, we shall be able to calculate this presumed extinction of light. The fraction representing the light lost would be $\frac{1}{100}$ for first magnitude stars, $\frac{8}{100}$ for stars of the sixth magnitude, and $\frac{88}{100}$ for the faintest stars seen in Herschel's telescope. The stars beyond this limit, if as numerous as is supposed, would not contribute more than $\frac{12}{100}$ of their light to the Milky Way. It follows, therefore, from this, that Herschel had fallen into a fundamental error concerning the power of his telescope. It would not reach more than 369 times the mean distance of stars of the first magnitude, instead of carrying 2300 times that distance as had been expected, without taking account of the extinction of light.

[To be continued.]

*Radiant-points of the Minor Showers visible at the
Lyrid Epoch, April 17-25.*

I HAVE examined about 620 paths of meteors which I registered here in various years (1873 to 1904) at the April epoch, and sorted them to their radiant, which appear to be very numerous. Altogether about 100 different showers are indicated, and by far the larger proportion of these are exceedingly feeble and do not supply one meteor in 10 hours' watching by a single observer. In fact, the meteoric streams of April afford another instance of what is sufficiently apparent at all the principal periods of the year, viz., that there are vast numbers of systems visible which are remarkable for their tenuity. Some of the supposed centres of radiation

appear to be indicated by isolated meteors, and to show that if these bodies do not revolve singly in space they belong to, or are the relics of, such exhausted systems that, though an observer may watch the sky during the whole of several successive nights, he will probably not catch a single fragment of such systems.

Under these circumstances, the utility of several observers at different stations looking out simultaneously, so that duplicate observations of the same objects may be secured, becomes obvious. In this way the radiant positions may be determined in cases where the streams are far too feeble to enable one observer to ever obtain sufficient data for the purpose. A number of radiants have already been ascertained in the manner referred to, and a great many still remain to be found; and it is hoped that this circumstance will prove an incentive to further persevering observations at the April period, which is an exceedingly interesting one, though meteors generally (exclusive of the Lyrids) are decidedly scarce.

The following are the principal radiants and some of those feebly suspected, from the observations at Bristol:—

Radiant-points of the April Period, April 17-25.

° ° Meteors.		° ° Meteors.		° ° Meteors.	
160	+59..... 6	232	+38..... 6	272	+21..... 17
165	- 6..... 6	234	+10..... } 13	290	+53..... 8
189	-31..... 6	236	+ 6..... 5	290	+60..... 5
189	+58..... 6	235	-15..... 5	290	+70..... 5
200	+ 9..... 10	239	+46..... 14	293	+43..... 5
203	-10..... } 20	244	+63..... 5	296	± 0..... 6
218	- 5..... } 20	244	+72..... 5	300	+22..... 15
203	-57..... 7	247	+ 1..... } 12	308	+61..... 5
213	+53..... 13	248	+10..... } 12	315	+78..... 8
218	+33..... } 23	254	+57..... 7	310	+35..... } 10
219	+39..... } 23	255	+49..... 8	318	+31..... } 10
221	+13..... 10	253	-20..... 5	316	+48..... 12
228	- 2..... 11	260	+62..... 18	327	+48..... 5
230	+27..... 10	262	+36..... 18	334	+58..... 5
228	+33..... 5	271	+33..... 148	334	+71..... 5
231	+17..... 14	271	+47..... 5		

Tenuous showers very slightly indicated.

° °	° °	° °	° °
22 +59	198 +20	245 +35	280 -14
30 +36	206 +18	254 +37	301 +40
50 +43	*218 -31	256 +25	303 +54
121 +28	213 + 9	276 +11	312 +15
190 +20	220 +27	274 +67	330 +28
194 +67	228 +53	280 +47	330 +37
195 +45	236 +61		

* This has formed a pretty active shower in recent years as seen by other observers, though only a few of its meteors appear to have been recorded at Bristol.

Bishopston, Bristol,
1904, April 22.

W. F. DENNING.

CORRESPONDENCE.

*To the Editors of 'The Observatory.'**The Annular Eclipse of December 30, 1777.*

GENTLEMEN,—

The total eclipse due on the 9th of September next is very disobliging to astronomers, as the central line will cross land only over some small islands of the Pacific Ocean. Nevertheless, remembering what was achieved on Caroline Island in 1883, it is strange that no attempt seems likely to be made for its observation. The place which would probably give most hope of success is Christmas Island, situated in W. long. $157^{\circ} 27'$, and N. lat. $1^{\circ} 57'$. It was so named by Capt. Cook because he spent his Christmas there on his famous and fatal last voyage, undertaken with the object of discovering a passage from the Pacific to the Atlantic, in 1777. He remained at anchor there for more than a week, on account of the approaching eclipse of the Sun, which was annular about 20° further to the south, and therefore large, though partial, on Christmas Island. Accordingly, he and some of his party landed there on the 28th and again on the 30th, when the eclipse, already prepared for, was to take place in the morning. The sky was overcast till past 9 o'clock, when in a clearance they took the Sun's altitude and rectified their time. Then the sky clouded again till 30 minutes past 9, when it was found that the eclipse had already commenced. However, measures were obtained of the uneclipsed part of the disc, at which observations, says Cook, "I continued about three-quarters of an hour before the end, when I left off; being in fact unable to continue them longer, on account of the great heat of the Sun, increased by reflection from the sand" ('Voyage to the Pacific Ocean,' vol. ii. p. 184). The Sun, he goes on to say, was clouded at times, but the sky was clear at the end of the eclipse (nearly half an hour after noon), the time of which was noted by Mr. Bailey, Mr. King, and the Captain himself, Mr. King using a reflector and the others each a large achromatic, of the same magnifying-power. The times given by each are:—

C	0 ^h 25 ^m 37 ^s
B	0 26 3
K	0 26 1

Commenting on his own register being so much earlier than the others, Cook says:—"Perhaps it was in part, if not wholly, owing to a protuberance in the Moon, which escaped my notice but was seen by both the other gentlemen." It is difficult to understand what this could have been; had it been seen at the end of totality of an eclipse it might have been a solar protuberance, but none such could have been seen at the end of an eclipse never more than partial where observed.

Cook left the island on the 2nd of January, 1778. He judged it to be about 15 or 20 leagues in circumference, and of a half-moon shape, the two horns being the north and south points. There was no trace that any human being had been there before them. Abundance of fish and turtle was to be had, but no fresh water could be found, though frequently dug for. There were several salt-water ponds, which must have been filled by water percolating through the sand from the sea at high tides. It is now used as an American guano station. Should an observing-party make use of it next September, they will only land as long as necessary for the observation, but may remember that, if successful, it will not be the first eclipse seen from the island.

Yours faithfully,

W. T. LYNN.

Blackheath, 1904, May 31.

P.S. (June 13).—It does not seem quite certain whether Christmas Island will be within or just outside the shadow-track next September, but at any rate the eclipse will be very large there, and almost, if not quite, total.

Schroeter and the Burning of Lilienthal in 1813.

GENTLEMEN,—

Mr. Lynn in his interesting letter on "The third Small Planet" in your number of last March incidentally mentions Schroeter and the plundering and burning of his observatory at Lilienthal near Bremen in 1813. In 'The Northern Campaigns' by Hemingway, the following letter written by Schroeter is quoted:—

A short time before the French were driven from this country (on their retreat before the Russians in January), between 20 and 30 Cossacs being quartered at Lilienthal, about 200 French soldiers ventured to come hither; the Cossacs drove them from the dam of the little river Worpe into the houses, but having no room to manœuvre retreated to a short distance. Four Cossacs, however, crept along the ground, and firing among them through the open house-doors, forced them to fly through the Worpe; they kept firing upon them out of the cottages, wounded an officer and pursued them into Borgfeld, from whence, on the same day, they retreated to their headquarters at Ottersberg. The wounded officer asserted that the detachment had been fired upon by the peasantry from the cottages. Without any further notice, therefore, General Vandamme gave orders to set fire to Lilienthal, with all the small villages adjoining, and the government buildings; at the same time the French poured a shower of bullets into the houses to prevent the poor inhabitants from saving their goods and fired at all those who ventured to carry off anything. Two were killed and many wounded. The barbarians having executed this horrible vengeance now first instituted an examination and pronounced the whole town of Lilienthal and adjoining villages *innocent*! The damage done is computed at 60,000 rix dollars (about £12,000). A strong wind aided the conflagration. Not a scrap of paper did I save; but was obliged to fly with my family, in our night dresses, to my farm at Adolphsdorf. I have suffered a loss in corn, hay, straw, and moveables to the amount of 9000 rix dollars and reckon my books at 6000. But I am still more hurt at what followed; about 6 days after, when I was obliged to be absent at Bremen, these vandals returned from Borgfeld to

Lilienthal, broke into the observatory, which had escaped the flames; and with a fury the most unprovoked and irrational destroyed or carried off the most valuable clocks, telescopes, and other astronomical and mathematical instruments. My servants afterwards bought a few of these articles from the robbers; and I got back a 3-foot achromatic telescope, which the *honest* officer of the gang had secured, but most of the other instruments were lost. The loss falls properly upon the University of Gottingen, but I am willing to consider it as my own. During the French administration I was obliged at least to build half of their public structures for them, but, as usual, my account remained unpaid. Many of the poor sufferers in this country, whom the French have robbed of their all, have applied to me, that I might represent their case, and procure some charitable relief for them in England by a collection among the benevolent people there, but I do not know whether the particular case of Lilienthal will excite much attention.

Schroeter also refers on Jan. 22, 1815, to his loss in his work on the great comet of 1811 as follows:—

At length after the most touching afflictions of mortality, I once more awake in my temple consecrated to the Eternal Godhead and am again able after a total derangement of my affairs to edit these collections concerning the great comet of 1811.—Through the most barbarous fury in consequence of an equally barbarous decision, the whole innocent soft vale of Lillies was burnt to the ground without any previous examination. They likewise burnt down the Royal Government buildings. I lost my whole moveable property and, what was most sensibly felt by me, amongst it, with a considerable loss also to the booksellers of Europe, the sole copy of the whole of my works and writings deposited in the Government house. Even my observatory, preserved by providence from the fire, was a few days afterwards broken into, plundered, and shamefully thrown into confusion by demolishing the clocks, breaking off the finders from the instruments, and carrying off the smaller instruments. Previously indeed, having been removed from my post, my income had gradually become so very straitened I was obliged to forego all but the most necessary outlays and to give myself up to a scientific slumber. Under the endurance of these troubles all my scientific patrons and friends will doubtless, as far as possible, excuse me, if, through melancholy and on account of the extraordinary high rate of postage, I have been compelled to put out of sight so many obligations of courtesy; for to the present time everything is so straitened with me that my observatory from want of time and heavy expenses is for the most part a confusion.

Schroeter did not long survive the losses and inconveniences to which he was subjected by the wars of the period. He died on 1816, Aug. 29, when 70 years old.

Bristol, 1904, July 4.

Yours faithfully,

W. F. DENNING.

Jupiter apparently Moonless.

GENTLEMEN,—

A perusal of the *Nautical Almanac* for 1907 indicates that this curious and somewhat unusual appearance is presented for the first time in the twentieth century on October 3, 1907, but, unfortunately, for a few minutes only.

Many will remember the last occasion when this was seen, on August 21, 1867, on which occasion Jupiter appeared unattended for an hour and three quarters. The previous occasion was on September 27, 1843, when the disappearance was for half an hour.

But the following is given as the configuration of the satellites on March 21, 1874:—

	h	m			h	m	s
I. Tr. E.	0	6		I. Oc. D.	19	4	
I. Sh. E.	0	12		IV. Ec. R.	20	57	13
III. Oc. D.	+ 17	53		II. Tr. E.	21	11	
IV. Oc. D.	18	6		III. Ec. R.	21	20	55
II. Tr. I.	18	35		I. Ec. R.	21	24	9
II. Sh. I.	18	46		II. Sh. E.	21	26	

∴ from 19^h 4^m to 20^h 57^m there would be a disappearance. I do not remember having seen account of observation by any astronomer in the southern hemisphere of what seems to have been the longest disappearance of the satellites.

On the morning of October 15, 1883, as seen through a small telescope, Jupiter appeared denuded of his satellites for about half a minute. Through a large instrument this would not be an entire disappearance. Had the tables of the IV. sat. been perfect, there would have been a disappearance for more than a quarter of an hour.

On the 3rd of October, 1907, the configurations are: II. Tr. I. 7^h 46^m; III. Ec. R. 7^h 56^m 26^s; the other satellites not being outside the disk of the planet at the time.

Melplash Vicarage, Bridport,
1904, July 15.

Faithfully yours,
S. J. JOHNSON.

Kaspar Gottfried Schweizer.

GENTLEMEN,—

The death of Prof. Brédikhine on the 14th of May last leads us to devote a few thoughts to the earlier history of the observatory at Moscow, of which he had so long been Director. Continuous activity there began in the year 1858, soon after the appointment of his predecessor, Kaspar Gottfried Schweizer. That astronomer was born on the 16th of February, 1816, at Wyla in the canton of Zürich. After he had completed his studies at Zürich, he went in 1839 to Königsberg to assist Bessel, and two years later accepted a similar post under the elder Struve at Pulkowa. He first went to Moscow in 1845, and was appointed Professor of Mathematics and Astronomy at the Survey Institute in 1852, and Director of the Observatory in 1856. He was the first discoverer of the third comet of 1847 (on August 31), of the third comet of 1849 (on April 11), of the second comet of 1853 (on April 4, it was visible to the naked eye early in May), and of the first comet of 1855 (on April 11); besides being an independent discoverer of the first comet of 1853, which he observed on the 8th of March (as also did Tuttle), but was anticipated by Secchi, who saw it two days earlier. In 1851 he was sent on an expedition to observe the total eclipse of the Sun,

which took place on the 28th of July in that year. The station selected was Machnowka, in the government of Kiev, where the weather turned out to be not all that could be desired, so that nothing worthy of note was accomplished. Schweizer was a diligent observer of solar spots and other phenomena; in 1853 he communicated a paper to the *Astronomische Nachrichten* (vol. xxxvi.) on the probable identity of the protuberances and faculae; also of meteoric appearances and of that peculiar oscillatory movement in the stars which was first noticed by Humboldt at Teneriffe in 1799, and may be subjective or may arise from a similar cause to that which produces scintillation. In 1862 and 1864 he published papers on the observed effects of local attraction in and near Moscow. His last publications were on determining the direction of a meridian line in geodetic operations, which appeared in 1870 and 1871; and he died at Moscow on the 6th of July, 1873.

Yours faithfully,

Blackheath, 1904, July 8.

W. T. LYNN.

Eclipse of April 10, 1679.

GENTLEMEN,—

On the above date there seems to have been an eclipse across Ireland, total with the Sun in the horizon. It seems too much to expect that any astronomical, meteorological, or historical work can be found containing an account of the phenomenon or of the gloom. In the year 1230, on May 14, the Sun was totally eclipsed at sunrise a little north of London, and of this earlier eclipse there is an account given by Roger of Hoveden.

This makes three solar eclipses across Great Britain so near to our days as the 17th century, of which we have no mention of the total phase. The dates are August 12, 1654, total about Inverness; that of 1679, and that of September 23, 1699, total, probably, for a few seconds across Caithness. The celebrated eclipse of 1715 was a return of that of April 1679, after two periods.

I am, Gentlemen,

Melplash Vicarage, Bridport,
1904, July 15.

Faithfully yours,
S. J. JOHNSON.

Erratum in the Nautical Almanac for 1904.

GENTLEMEN,—

I am indebted to Mr. Walter Heath, F.R.A.S., for his kindness in pointing out to me a discrepancy between the predicted and observed times of the occultation of θ^2 Tauri by the Moon on the 9th instant. I find that the mean place of this star, as given on page 491 of the *Nautical Almanac*, is erroneous. It had been assumed that the Annual Catalogue of Stars, as printed in the volume of 'Greenwich Observations' for 1897 (from which the adopted place of θ^2 Tauri for 1904.0 was deduced), was for the

epoch 1897.0 in accordance with the custom of past years. It appears, however, that a change was made in this volume, and that the Annual Catalogue is brought up to the epoch 1900.0. As the star is again occulted at Greenwich on September 29 of this year, I ask permission to give the corrected times of occultation on that date to be substituted for those on page 539 of the *Nautical Almanac*.

Mean Time of Disappearance.....	13 ^h 43 ^m
Mean Time of Reappearance	14 54

Similarly the time of appulse on December 20 should be 6^m earlier than the printed time.

Yours faithfully,

H.M. Nautical Almanac Office,
1904, July 12.

A. M. W. DOWNING.

OBSERVATORIES.

CAMBRIDGE UNIVERSITY. — Sir Robert Ball's Report to the Observatory Syndicate gives some pieces of information that have already been mentioned in these pages: among them the appointments of Mr. Newall as Assistant Director and Mr. Hinks as Chief Assistant *vice* Mr. Graham retired. Mr. W. E. Hartley, M.A., of Trinity College, has been appointed Second Assistant in place of Mr. Hinks; Miss Walker, who has been computer for 22 years, has resigned; and two other ladies, Miss Bell and Miss Malden, have been attached to the Observatory as computers during the year. Mr. H. N. Russell is an honorary member of the staff.

The principal work of the Observatory at present is the reduction of the Eros photographs, which has already been described in this magazine. Mr. Russell is using the Sheepshanks Photographic Equatorial in the determination of stellar parallax. His working list includes 59 fields. Up to 1904 May 24, 100 plates had been obtained of 45 fields; 64 plates of 29 fields had been completed, measured, and some of these had been reduced.

The retirement of Mr. Graham and of Miss Walker makes a break in the work of the meridian circle. Opportunity was taken to overhaul this instrument and make several improvements. A printing arrangement have been added to the declination micrometer, so that it is possible to make three settings at each transit; a new setting-wheel and telescope has been added, so that the instrument can be conveniently set from the eye-end. The silver heads of the circle microscopes have been replaced by white celluloid, and other modifications have been made. This instrument, under the charge of Mr. Hartley, will now be used for making the observations for the Zodiacal Catalogue.

The meteorological work is being put on a new basis. The instruments, which were those of a second-class station, were

transferred to the Botanic Garden at the end of last year, and it is proposed that in place of this work some special researches shall be undertaken. A Bendorf electrograph with Radium radiator, a Dines self-recording Barograph, and an equatorially-mounted Callendar sunshine-recorder have been set up.

Mr. Newall's Report states that the instrument which bears his name has been used, as in previous years, for spectroscopic researches, chiefly for determining stellar velocity in the line of sight. Observations of stars selected in co-operation with other observatories on the Continent and in America have been continued, and the reduction of these made in 1903 is being completed for publication.

NOTES.

COMET NOTES.—Prof. J. Rheden, of Vienna, has computed the definitive orbit of Comet 1890 III. (Coggia), which was only under observation from July 18 to August 13, 1890. The positions and proper motions of the comparison stars are investigated. The orbit which gives the smallest residuals is the following hyperbola:—

T	1890 July 8·6057 Berlin M.T.
ω	85° 43' 49" } 1890·0.
Ω	14 16 53 }
i	63 18 23 }
$\log q$	9·88360
a	131·27
e	1·0058

A parabola, however, will represent the observations with moderate residuals, so the hyperbolic nature of the orbit is uncertain.

MM. Kaminsky and Ocoulitsch have computed approximate elements of Encke's Comet for its approaching return. Only the Jupiter perturbations, and these merely to the first order, are included:—

Epoch and osculation 1904 Nov. 9·0 Berlin M.T.	
M	341° 3'·7 } 1904·0.
ω	184 35·5 }
Ω	334 27·1 }
i	12 35·6 }
ϕ	57 54·3
μ	1075"·7
$\log a$	0·34555
T	1905 Jan. 11·37 Berlin M.T.

Ephemeris for Berlin Noon.

R.A.						N. Dec.		R.A.						N. Dec.	
h m s						° '		h m s						° '	
Aug.	11....	1	50	21	20	53		Sept.	30....	1	20	49	27	52	
	21....	1	52	57	22	20			Oct.	5....	1	8	31	28	17
	31....	1	52	39	23	48			10...	0	53	35	28	31	
Sept.	10....	1	48	18	25	16			15....	0	35	57	28	27	
	20....	1	38	20	26	41			20....	0	15	47	28	4	
	25....	1	30	39	27	19									

At the beginning of the ephemeris the comet may be just visible in large instruments; its brightness will increase very rapidly, and it will probably be a conspicuous object in October and November, perhaps just visible to the naked eye.

A. C. D. C.

MINOR PLANET NOTES.—The new planet OE was discovered by Dr. Wolf at Heidelberg on May 13 last. Another new planet, OG, of magnitude 12.0, was discovered there by Herr Gotz on July 14.

A. C. D. C.

A POSSIBLE VARIATION OF SOLAR RADIATION.—The following is a transcript of the summary and conclusions of a remarkable article by Prof. S. P. Langley in the *Astrophysical Journal* for June:—

“A series of determinations of the solar radiation outside the atmosphere (the solar constant)*, extending from October 1902 to March 1904, has been made at the Smithsonian Astrophysical Observatory under the writer's direction.

“Care has been exercised to determine all known sources of error which could seriously affect the values relatively to each other, and principally the varying absorption of the Earth's atmosphere. Though uncertainty must ever remain as to the absorption of this atmosphere, different kinds of evidence agree in supporting the accuracy of the estimates made of it and of the conclusions deduced from them.

“The effects due to this absorption having been allowed for, the inference from these observations appears to be that the solar radiation itself fell off by about 10 per cent., beginning at the close of March 1903. I do not assert this without qualification, but if such a change in solar radiation did actually occur, a decrease of temperature on the Earth, which might be indefinitely less than 7°·5 C., ought to have followed it.

“On comparing the observed temperatures of 89 stations, distributed over the North Temperate Zone, with the mean temperatures of the same stations for many previous years, it is found that an average decrease of temperature of over 2° C. actually did follow the possible fall of the solar radiation, while the temperature continued low during the remainder of the year. Stations

* I wish to renew my caution that the *absolute* values of the solar constant thus given are more likely to be found in defect than in excess.

remote from the retarding influence of the oceans show a much greater variation than that of the general mean.

"While it is difficult to conceive what influence, not solar, could have produced this rapid and *simultaneous* reduction of temperatures over the whole North Temperate Zone, and continued operative for so long a period, the evidence of solar variation cannot be said to be conclusive. Nevertheless, such a conclusion seems not an unreasonable inference from the data now at hand, and a continuation of these bolographic studies of solar radiation is of increasing interest, in view of their possible aid in forecasting terrestrial climatic changes, conceivably due to solar ones."

SUN-SPOTS AND HOT SUMMERS.—A singular effect of the outburst of sun-spots of last year was the crop of theories relating to the connection of sun-spots with weather. One writer, discreetly refraining from committing himself to a definite statement, declared that abnormal weather might be expected as a consequence of the solar outburst, "hot," "stormy," or "wet." In France it was confidently stated that the very brief season of warm weather experienced in the spring was due to the sun-spots; and this opinion has been recently revived by M. Flammarion, who attributes the heat of the present summer to the same cause. It would seem, therefore, that any unusual phenomenon may be conveniently ascribed to solar action as defined by the waxing and waning of sun-spots. The connection between magnetic changes and the well-defined eleven-year sun-spot period is no doubt well established, but other suggested connections are mostly fantastic and chimerical, and are generally pressed too far. Dr. Buchan, working on the lines initiated by the late Dr. Meldrum, considers that his researches prove that a maximum is shown in Scottish rainfall at the epochs of maximum sun-spots, a relation which has not been found to exist in the Greenwich record; and it may perhaps be profitable to look briefly into the supposed connection between sun-spots and hot summers. Defining a hot day as one on which the shade temperature rose to 80° , it is found from the observations made at the Royal Observatory that the greatest number of such days occurred in the following years:—in 1868, 40; in 1857, 35; in 1859, 34; in 1846, 32; in 1893, 29; and in 1876, 28. The maximum sun-spot epochs, as derived from Dr. Wolfer's revised table of Wolf's sun-spot numbers, are 1848, 1860, 1870, 1883, and 1894. The agreement with the hot summer years is not striking. This is confirmed by the following record of absolute annual maximum temperatures, viz.:—

1881		97.1
1868		96.6
1893		95.1
1858		94.5
1884		94.2
1876 and 1900		94.0

and also by the highest mean annual temperatures and excesses above average, which are as follows :—

	Temperature.	Excess.
	°	°
1868	52°0	+2°5
1898	51°4	+1°9
1846	51°4	+1°9
1857	51°3	+1°8
1859	51°2	+1°7
1872	51°0	+1°5
1893	51°0	+1°5

From the results of these observations it would appear that the connection between maximum period of sun-spots and warm weather is not established.

W. C. N.

THE WEATHER IN JULY.—In the first part of the month it seemed that the abnormally dry June was to be followed by an equally dry July, for up to and including the 24th of the month the total rain recorded at Greenwich only amounted to about 0·4 inch, the fifty-years' average for the whole of July being 2·47 inches. The storm came in the evening of Monday, July 25, and the record for the 24 hours ending 9 A.M. on July 26 was 1·546 inches. The rainfall was most rapid about 6^h 45^m in the evening of July 25, when three-quarters of an inch fell in a quarter of an hour. Lightning flashes were frequent and brilliant, and several casualties were recorded in the neighbourhood of Greenwich; but the storm appeared to be curiously local, for the rainfall for the day in London was only about half an inch, and there was very little rain at Eltham. The number of hours of bright sunshine up to the night of July 26 amounted to 256·3. The average for the whole month is only 185 hours.

THE OCCULTATION OF ALDEBARAN.—This phenomenon was well observed at Greenwich on July 9, as well as occultations of several stars in the Hyades, through which the Moon passed on that night. Aldebaran disappeared at the illuminated limb, which was only faintly visible, as it was full daylight, but there was very little evidence for "projection on the limb." It was observed by four observers, who all stated independently that the disappearance was sudden. Two of them thought the star encroached very slightly on a limb, the third observer thought it did not.

THE DETROIT OBSERVATORY.—The Detroit Observatory of the University of Michigan, sometimes known as the Ann Arbor Observatory, was built about 1854, through the efforts of President Tappan, who raised the money. The equipment consisted chiefly of a meridian circle called the "Walker," after the name of the man who gave the funds to buy it, and a 12 $\frac{3}{4}$ -inch equatorial, and with the latter Prof. Watson, who was one of the earliest

Directors, discovered twenty-one asteroids, and used it possibly for the construction of his ecliptic charts. The meridian circle was used by Mr. Schaeberle, who was then on the staff, but little seems to have been done with it, although it is said that Brünnow made with this instrument an elaborate series of observations of the Bradley stars, the records of which he brought to Europe for reduction. But these seem never to have arrived at publication.

Prof. Asaph Hall, jr., is now Director of the Observatory, and when he took charge about seven years ago he determined to take up meridian work and overhauled the instrument. Several vital faults were soon brought to view. The instrument is supported on two brass cones let into stone piers and packed therein with mortar mixed with brick-dust, a cement which was formerly recommended by text-books, but in this case the cement was found as a fine dry dust and quite inefficient, also the object-glass cell fitted loosely in the telescope-tube, so that its weight came entirely on three collimating screws, which it was suspected could not stand the strain. These faults having been remedied and other improvements made, Prof. Hall looked around for some investigation to make, which would be a test of capability of the instrument, and elected to observe zenith-distances of Polaris with the idea of determining the aberration constant. The result of this work depending on observations made in the years 1898, 1899, and 1900 is published in Part I. of the Transactions of the Observations recently issued. It is scarcely possible to give details of the work here, but it is pointed out that some of the residuals are large, and there are some indications of sudden changes in the structural condition of the instrument. The value of the aberration constants deduced from the observations is $20''.683$, an exceptionally large value.

SPECTROSCOPIC OBSERVATION OF STELLAR RADIAL VELOCITY.—Bulletin No. 11 of the Lowell Observatory gives details of the observations of some stars which may be binaries. In the spectrum of α Andromedæ the displacements of the hydrogen H γ line indicate a velocity in the line of sight ranging from -45 to $+20$ kilometres per second, which is not inconsistent with motion in a very eccentric orbit in a period of about 100 days. α Libræ shows velocity varying from -60 to $+20$ km., α Scorpii from -25 to $+25$ km. per sec. χ Sagittarii, whose brightness varies in a period of 7 days, has a small but well-marked change of radial velocity. Observations of ϵ Capricorni also show some variation.

Mr. W. S. Adams, of the Yerkes Observatory has determined the radial velocity of several bright stars in the Pleiades, with the following results:—

Electra	+ 14 km.	Merope ..	+ 6 km.
Taygeta ..	+ 3 „	Alcyone ..	+ 15 „
Maia	Variable.	Atlas	+ 13 „

These are given in the *Astrophysical Journal* for June, and

Mr. Adams adds that emphasis should not be laid upon the character of measures of spectra of this nature. It is certainly not too much to say that ranges of at least 5 km. between successive plates of the same star or between duplicate measurements of the same plate may well be expected in the case of any of these stars.

NEW OBSERVATORY.—The Royal Academy of Science of Barcelona has established the Fabra Observatory. The institution, which was formally inaugurated by the King of Spain on April 7 last, contains an astrographic equatorial having an object-glass of 38 centimetres and a meridian circle having an objective of 20 cm. diameter. Sr. J. Comas Solá is the Director.

THE CAIRO OBSERVATORY.—On January 1st, 1904, the Observatory, which had hitherto been situated at Abbassia on the north side of Cairo, was transferred to a new site at Helwan, which is the health-resort on the desert plateau about 22 kilometres south of Cairo. The buildings are on the limestone rock, which here forms the surface of the desert. At present the main building is occupied and the meteorological self-registering equipment is installed there. There is a Transit house and an Equatorial house. This observatory is responsible for the dropping of the Time-balls at Port Said and Alexandria.

THE following speech was delivered by Prof. Love when presenting Sir David Gill for the degree of D.Sc. *honoris causa* at Oxford on June 22 :—

In extremo Africæ meridionalis promontorio sub tutela navium Britannicarum surgit turris ad siderum motus observandos destinata. Illic plus viginti iam annos magno astronomorum omnium emolumento cæli signa perscrutatus est David Gill. Illic rem quater de integro aggressus id adsecutus est ut distantiam, quæ inter solem et nostram terram intercedat, accuratius quam quivis e prioribus emetiretur, adeo ut hodie omnes astronomi eius rationes pro veris habeant. Idem eodem modo distantiam inter solem et quindecim ex stellis, quæ fixæ vocantur, definivit. Accuratissimam etiam descriptionem fecit earum cæli regionum, quas non nisi Australe latus orbis terræ incolentibus spectare concessum, et in tabulis maximis faciendis, quibus variae cæli partes cura exquisitissima depinguntur, cui operi præclaro omnes ubique gentes hodie incumbunt, rem felicissime navavit.

UNDER the auspices of the Optical Society it has been arranged that an Optical Convention shall be held in London in April or May of next year. The Vice-Presidents, one of whom will be chosen as President, are :—The Earl of Crawford, The Earl of Rosse, Lord Kelvin, Lord Rayleigh, Lord Blythswood, Sir Wm. Abney, Sir Howard Grubb, Mr. Alban Gibbs, M.P., The Astronomer Royal, Mr. Dallmeyer, Mr. Stuart, and Dr. Glazebrook.

THE Memorial to Sir George Stokes, which takes the form of a medallion relief-portrait in Westminster Abbey, was unveiled by the Duke of Devonshire on July 7. Sir William Huggins, Sir Norman Lockyer, and Prof. Larmor were present, amongst others.

PROF. H. C. WILSON in *Popular Astronomy* discusses the subjective existence of the large nebulous areas of the sky described by Sir Wm. Herschel. He thinks that the late Dr. Roberts's conclusions that these were non-existent depend on imperfect evidence.

THE University of Durham has conferred an honorary degree of D.Sc. on Prof. R. A. Sampson, F.R.S.

FROM AN OXFORD NOTE-BOOK.

THESE lines are written during a most peaceful voyage across the Atlantic in the good ship *Devonian*. The sun shines, the breeze blows just enough to keep us cool, the boat is far from crowded and very steady, and the only possible complaint one can make is that the conditions are so exceptional that it is quite easy to work. On most voyages work is all but impossible, and there is no choice but to take a complete holiday; and on eclipse expeditions this holiday is extremely welcome between the two busy times of preparation—firstly before starting, and secondly at the eclipse stations. But in this glorious weather there is a continual small battle with conscience, though conscience is apparently rather weakened by sea-air; and, further, beneficent members of the other sex chatter so ubiquitously that it becomes easy to convince the enfeebled monitor that there is too much noise for real mental application.

HENCE it has happened that even one or two matters which had been jotted down to think over on the voyage have not been very seriously examined. One of them was Schaeberle's recent remark about a possible small refraction of rays of light from stars on entering the solar system, which *might* explain, to some extent, the small absolute parallaxes of the stars. The notion seems well worthy of consideration. If we suppose that the Sun is the centre of a homogeneous sphere of refractive index $1+\mu$, and radius R , a good deal larger than the Earth's orbit, then a star at distance R/μ would show no parallax when viewed from opposite points of the Earth's orbit; and stars beyond this would show a negative parallax. The stars may therefore not be so far away as they seem. Of course relative parallaxes would not be affected, and no very great weight has been thrown on to the few determinations of absolute parallax hitherto made. Perhaps the best test of Schaeberle's hypothesis will come from the comparison of observed velocities in the line of sight with proper motions, which should ultimately give an approximate idea of absolute parallax. But I cannot profess to have paid the hypothesis the attention it deserves as yet, for the reasons above indicated.

IN such weather it is pleasanter to follow some one else's thoughts than to think oneself. One specially fortunate day,

glancing at the ship's library, I caught sight of Leslie Stephen's 'Playground of Europe,' a book I had always wanted opportunity to read; and there could scarcely be a better opportunity than this peaceful time at sea. Mankind, they say, is divided into two camps, those who love the mountains and those who love the sea; and the implication seems to be that the devotions are incompatible. Leslie Stephen himself adopts this view; for he apparently thinks that it is necessary, in order to do full justice to his beloved mountains, to disparage the Atlantic, which "even in repose is merely suggestive of the absence of the most undignified of all ailments" (to quote from memory a phrase I cannot find again). But I humbly submit as one way of testing the delights of both simultaneously the reading of Leslie Stephen's word-pictures in mid-Atlantic under conditions which would surely have drawn from him something more than a negative expression of contentment.

THE book is so well known that it seems almost absurd to make extracts from it; except that there are probably others like myself who have lived many years without reading it, and may be glad of some small incentive towards repairing the omission. With proper apologies, therefore, I will quote a portion of the splendid account of "Sunset on Mont Blanc," which has a reflected astronomical interest. The chapter opens with a vivid description of the immensity of the "old monarch," which is particularly manifest when the surrounding country is viewed from the summit, just *before* sunset, for then the evening shadows throw the detail into relief, and the observers "not only knew but felt that at their feet was lying a vast slice of the map of Europe." And then after a beautiful description of the sunset glow comes this passage:—

And suddenly began a more startling phenomenon. A vast cone, with its apex pointing away from us, seemed to be suddenly cut out from the world beneath: night was within its borders and the twilight still all round; the blue mists were quenched where it fell, and for the instant we could scarcely tell what was the origin of this strange appearance. Some unexpected change seemed to have taken place in the programme; as though a great fold in the curtain had suddenly given way, and dropped on to part of the scenery. Of course a moment's reflection explained the meaning of this uncanny intruder: it was the giant shadow of Mont Blanc, testifying to his supremacy over all meaner eminences. It is difficult to say how sharply marked was the outline, and how startling was the contrast between this pyramid of darkness and the faintly lighted spaces beyond its influence: a huge inky blot seemed to have suddenly fallen upon the landscape. As we gazed we could see it move. It swallowed up ridge by ridge, and its sharp point crept steadily from one landmark to another down the broad Valley of Aosta. We were standing, in fact, on the point of the gnomon of a gigantic sundial, the face of which was formed by thousands of square miles of mountain and valley. So clear was the outline that, if figures had been scrawled upon glaciers and ridges, we could have told the time to a second; indeed, we were half inclined to look for our own shadows at a distance so great that whole villages would be represented by a scarcely distinguishable speck of colouring. The huge shadow, looking even more strange and magical, struck the distant Becca di Nona, and

then climbed into the dark region where the broader shadow of the world was rising into the eastern sky. By some singular effect of perspective, rays of darkness seemed to be converging from above our heads to a point immediately above the apex of the shadowy cone. For a time it seemed as if there was a kind of anti-sun in the east, pouring out not light, but deep shadow as it rose. The apex soon reached the horizon, and then to our surprise began climbing the distant sky. Would it never stop, and was Mont Blanc capable of overshadowing not only the Earth but the sky? For a minute or two I fancied, in a bewildered way, that this unearthly object would fairly rise from the ground and climb upwards to the zenith. But rapidly the lights went out upon the great army of mountains; the snow all round took the livid hue which immediately precedes an Alpine sunset, and almost at a blow the shadow of Mont Blanc was swallowed up in the general shade of night. The display had ceased at its culminating point, and it was highly expedient for the spectators to retire. . . .

To the east the night rushed up furiously, and it was difficult to imagine that the dark purple sky was really cloudless and not blackened by the rising of some portentous storm. That it was, in fact, cloudless, appeared from the unbroken disc of the full Moon, which, if I may venture to say so, had a kind of silly expression, as though it were a bad imitation of the Sun, totally unable to keep the darkness in order.

With how sad steps, O Moon, thou climb'st the sky,
How silently and with how wan a face.

As Sidney exclaims. And truly, set in that strange gloom the Moon looked wan and miserable enough; the lingering sunlight showed by contrast that she was but a feeble source of illumination; and, but for her half-comic look of helplessness, we might have sympathised with the astronomers who tell us that she is nothing but a vast perambulating tombstone, proclaiming to all mankind in the words of the familiar epitaph, "As I am now you soon shall be!"

THE following extract from the *Daily News* of June 23, kindly sent me by a correspondent, seems worthy of reproduction in full:—

A NIGHT ON THE EIFFEL TOWER.

(From Our Own Correspondent.)

PARIS, Wednesday.

I was one of four hundred and fifty guests invited by the French Astronomical Society to pass the night on the Eiffel Tower—on "Tuesday, the 21st of June, date of the Summer Solstice."

The gathering was the first of the sun festivals, which the Astronomical Society, inspired by its poetically tempered ex-President, M. Flammarion, has resolved to perpetuate. A revival of sun worship, some of the papers have been calling it. It is simply a scientific festival, with the propagation of popular interest in the wonder and beauty of the universe as one of its chief motives.

We were all congregated on the first landing of the tower, when the crash of a gun down below, on terra firma, announced that the Sun had at that moment reached the solstice. Time, twenty-eight seconds past nine.

Then we went into the theatre—one of the numerous structures on the first landing, whose area is large enough to hold at least 8000 persons. The venerable President of the Society, M. Janssen, welcomed his guests, and read a short discourse, after which M. Flammarion delivered an eloquent address on sun worship in the world's past, on the Sun's influence in the stellar system, on astronomical discovery.

The address was followed by a great many beautiful illustrations projected on a white screen. After that there were hymns to the Sun, invocations to the Sun, prayers to the Sun, from composers and poets, sung and repeated by artists from the Opéra, the Comédie-Française, the Opéra Comique. M. Mounet-Sully was one of the reciters.

A luxurious "luncheon" at midnight followed. Half an hour after midnight the huge lift was ready to carry to the top of the tower all who wished to wait for sunrise. Some fifty of us went to the top, and remained there till four o'clock this morning.

Nearly a thousand feet below the Seine shone dimly. The lights of Paris looked as if the firmament itself had exchanged the heavens for the earth.

PHOTOGRAPHY is difficult enough in astronomy, where the objects to be photographed are sure to assemble in proper and complete groups at the right time, and we have only to pray for weather and apparatus to be propitious. We may therefore spare a sigh of sympathy for those who have to lure their wayward subjects before the camera. The subjoined legend, which reached me the other day on the back of a post-card, has a touch of real pathos :—

OXFORD UNIVERSITY JUNIOR SCIENTIFIC CLUB.

BALLIOL, May 10th, 1904.

The Club will be photographed on Monday, May 30th.

In order that the photograph may be a representative one, lunch will be first provided, free of charge, for the convenience of Members, in Balliol College Hall, at 1.15 P.M.

If you intend to be present, kindly sign this card and post it to the Secretary.

Member's Signature.....

THERE is much that is obscure about the following extract from the *Yorkshire Evening Post* of March 29. One wonders whether *spheroid* and *asteroid* were forgotten, or whether they were excluded by some consideration not mentioned. The *Times* report did not seem to make the case any clearer :—

WORD-MAKING IN COURT.

In the Court of Appeal, yesterday, some discussion took place as to the use of fancy words in trade cases. Mr. Moulton, K.C., said the word "*tabloid*" was a gift to the language, the plaintiffs reserving to themselves its special use. He did not know of a single word in the English language ending in "*oid*" which was not a fancy word. For instance, there was "*dermatoid*." Lord Justice Vaughan Williams: What do you say of "*waroid*" — a sort of war? (Laughter.)

In the extracts from Herbert Spencer's Autobiography given by the daily papers, far and away the best story, to my mind, was the one of Huxley. There are probably some who did not see it, and those who did will doubtless forgive its reproduction here :—

To those who knew him [Huxley] simply as scientific lecturer and writer, he presents only the graver side of his character; though reports of his after-dinner speeches might show even these that he has a fund of humour. To his friends, however, he is known as a sayer of good things—some of them things which, though forgotten by himself, are remembered by others. A witticism of his at my expense has remained with me these twenty years. He was one of a circle in which tragedy was the topic, when my name came up in connection with some opinion or other; whereupon he remarked—"Oh! you know, Spencer's idea of a tragedy is a deduction killed by a fact."

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- (6th.) Karrusel Watches (of which he is the Inventor, as well as sole maker of all Movements of the Karrusel construction so far on the Market, a good stock of which is always kept).
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A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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Meeting of the British Association at Cambridge, 1904.—
Subsection: Astronomy and Cosmical Physics. Extracts
from the Address by Sir JOHN ELIOT, K.C.I.E., M.A.,
F.R.S., Chairman of the Subsection.

THE Address consisted of three parts, viz. :—

1. A brief sketch of the broad features of the meteorology of India in their relations to the general meteorology of the Indo-Oceanic region.
2. Statement of abnormal features of the meteorology of that area for the unique period 1892-1902 illustrating the remarks in the preceding sketch.
3. Suggestion of the co-ordination of the meteorological observations of the British Empire and the creation of a central office for the investigation of problems of general meteorology.

The latter part we give in full.

India is the most typical example of monsoon conditions, that is, of opposite air-movements of six-monthly period which, in its case, depend on the annual temperature-changes in the sea and land areas of the Indian Ocean and continent of Asia. The monsoon conditions in India are intensified by its unique position and topography. It projects southwards into the Indian seas over 15° of latitude, and is protected northwards by the vast barrier of the Himalaya Mountain range and Thibetan plateau. The axis of the Himalayan range is at least 2000 miles in length and has an average elevation of over 20,000 feet. The extent of country over 10,000 feet in elevation to the north of India is from 300 to 500 miles in width. These figures will give some idea of the magnitude of India's northern barrier.

During one period of the year there is an outflow in the lower

atmosphere from land to sea. The direction of the lower air-drift in India is determined in part by the lie of the mountains and river-valleys, and is from north-east over the greater part of the Indian seas. January is the month most typical of this air-movement and of the accompanying weather conditions.

During another portion of the year the lower horizontal air-movement is from sea to land. This movement is much steadier and more powerful and influential in every respect than the former. July and August are the months most representative of the totality of the weather conditions of this period.

Conditions similar to those of January prevail in their entirety from about the middle of December to the end of February or middle of March—the period known in India as the cold-weather or cool season.

The hot-weather season lasts from about the middle of March to the middle of June (on the average in Northern India). During this period temperature increases rapidly until the last week in May or first week of June, when maximum day-temperatures ranging between 120° and 125° are usually recorded in the driest and hottest interior districts of Northern and Central India. Pressure decreases *pari passu* in the heated land areas of Southern Asia, which become areas of low pressure and indraught relative to the neighbouring seas.

The weather in India during this period depends almost entirely upon local thermal actions and contrasts of temperature and humidity conditions. Skies are generally free from cloud, but the air is more or less charged with dust and is excessively dry (humidities of 1 to 5 being of occasional occurrence in North-western India).

The characteristic features of the dry season are hence most strikingly exhibited immediately before the advent of the wet monsoon. There is no gradual change over the greater part of India from one to the other such as would occur if the furnace, or Central Asia hot area, theory were correct. Over small isolated portions of India, including Tenasserim, Arakan, Lower Burma, Assam, Bengal, and Malabar, thunderstorms giving more or less heavy downpours occur in increasing frequency during the period. The rainfall is considerable to large in amount in these areas, and is of much agricultural value in some districts—*e. g.*, in Assam for the tea crop. In those areas the transition to the rainy season is much less abrupt and spasmodic, the chief differences being that the rainfall in the wet season is more general and frequent, larger in amount, and rarely accompanies thunderstorms.

The transformation from the hot weather to the rains is usually effected between the 1st and 15th of June. It commences in the equatorial belt with a considerable increase of pressure and air-movement accompanying a strong rush of southerly winds, the continuation of south-east trades winds, across the equator. If the burst be sufficiently strong the rush is continued northwards

over the Indian seas as a wave of disturbance, squally weather, heavy rain, and much violent electric discharge or action, invading areas characterised previously by light and variable winds and fine weather. The disturbance usually increases with its northward advance, and frequently, when it reaches lat. 12° to 16° N., it concentrates into a cyclonic storm. Such a storm almost invariably marks the commencement of the monsoon in the Bay of Bengal, and in about two out of five years in the Arabian Sea. The advancing humid currents in the rear of these initial cyclonic storms or waves of disturbance march over the sea areas in a few days, and thence cross the coasts towards which they are determined by the low-pressure regions in the land areas of Southern Asia, where they produce an almost complete reversal or transformation of the weather conditions, the result of which is that moderately high temperature and small diurnal range of temperature, great humidity frequently approaching saturation, much cloud, and frequent rain obtain for the next three months over the greater part of India, until, in fact, the middle or end of September.

The reverse change—viz., the withdrawal of the humid south-west currents—then commences, and is a slow process, requiring usually from two to three months for its completion.

The year in India may hence be divided into two monsoons of nearly equal length, viz. :—

- (a) The north-east or dry monsoon.
- (b) The south-west or wet monsoon.

The first terms are based on the general direction of the air-movement in the Indian seas during the periods, and the second on the most prominent feature of the weather in India itself. Of an average annual total rainfall of 41 inches (according to the most trustworthy calculation), at least 85 per cent. falls during the wet season, and only 15 per cent. during the dry season.

The dry monsoon in India is subdivided into

- 1. The cold-weather period.
- 2. The hot-weather period or transitional period of preparation for the south-west monsoon.

The wet monsoon is subdivided into

- 1. The south-west monsoon proper, or period of general rains.
- 2. The period of the retreating south-west monsoon and gradual slow establishment of the dry monsoon.

Each of these periods practically covers three months.

The previous statement of the meteorology of India has indicated the chief conditions which affect the crop returns seriously or disastrously over large areas in India. They may be summed up briefly as follows :—

(a) The dry monsoon. Absence or unusual feebleness of cold-weather storms.

(b) The wet monsoon. General feebleness of the monsoon current, due either to corresponding feebleness of the south-east trades, or to unusual diversion to East Africa, or local feebleness in a part of India due to local conditions, or to abnormal diversion to other rainfall areas in South Asia: These conditions give rise in the areas affected to one or more of the following features:—

- (1) Prolonged delay in the commencement of the rains.
- (2) Scanty rainfall during the season, with prolonged periods of fine, clear, hot weather.
- (3) Early termination of the rains.

These features are as a rule more marked in the drier districts of the interior than in the coast districts. The effect on crop production is greatest and most disastrous in the following areas:—

- (1) Central Burma.
- (2) The Deccan, including the Bombay and Madras Deccan districts, and Hyderabad.
- (3) North-Western and Central India, more especially the South Punjab, East Rajputana, and the United Provinces.

The following important inferences are based upon the preceding presentation of facts and the experience of the past thirty years:—

(1) The lower air-movement of the south-west monsoon is the northward extension of the lower movement of the south-east trades. The latter is a permanent feature of the Indo-Oceanic Region, and the former a periodic invasion of the Southern Asian seas and peninsulas initiated over equatorial regions and propagated northwards to the southern mountain barrier of the Central Asian plateau.

(2) The primary factors determining this impulse across the equator (the first stage of the establishment of the south-west monsoon) are to be sought in the permanent field of the south-east trades, and are not due to actions in the heated areas of Southern or Central Asia.

(3) The pressure conditions in the heated areas of Southern Asia and North-East Africa determine the direction, volume, and intensity of the advance over the Indian seas to what may be termed three competing areas for rainfall (viz., Abyssinia, India, and Burma). These conditions are hence important factors in the third stage of the advance of the south-west monsoon current.

(4) The movement when fully established by these actions over the Southern Asian seas and peninsulas is continued—first, by the momentum of the lower circulation; secondly, by the release of energy accompanying aqueous vapour condensation; and thirdly, by thermal actions in Southern Asia, due to direct solar activity. The termination of the lower horizontal current by vertical movement occurs irregularly over the areas of frequent heavy rain in

Southern Asia and Abyssinia, and not over a heated area in Central Asia.

(5) The total volume of aqueous vapour brought up by this circulation not only varies in amount from month to month during the season, but also from year to year. The largest variations (seasonal and annual) depend chiefly, if not entirely, upon actions in the source of supply—viz., the Indian Ocean. If those actions determine an increased or diminished supply across the equator into the Indian seas, there is a corresponding variation in the total precipitation of the three competing areas. Amongst such causes and actions may be prolonged and untimely diversion of the south-east trades into East Africa, as in 1896; or general weakness of the air-movement over the Indian Ocean, probably accompanying a displacement and decreased intensity of the southern anti-cyclone, as in 1899.

(6) The relative distribution of the total rainfall in the three areas of discharge of the aqueous vapour of the monsoon currents probably depends upon the relative intensities of the pressure conditions established during the hot weather, which are continued for a part or the whole of the monsoon by actions depending on the rainfall resulting from the initial pressure conditions—an example of the persistence of meteorological conditions and actions which is a prominent feature of Indian meteorology. The total rainfall of each of the three areas may differ considerably from the normal, but there may be partial or complete compensation on the whole. Thus it is the general (but not the invariable) rule that the rainfall variations in Burma and Assam are usually inverse to those of North-Western India and also of India as a whole.

(7) The distribution of the rainfall in any one of the three competing areas (but more especially in India as the largest) may vary widely from the normal—considerable deficiency in some areas accompanying considerable excess in others. This in India is undoubtedly due to local conditions—*e. g.*, local excess or deficiency of pressure at the commencement of the period and established during the previous hot weather. These pressure variations usually accompany abnormally prolonged and heavy snowfall or very scanty snowfall in the Western Himalayas.

(8) Local or general drought in India during the south-west monsoon may hence be due to—

(a) General weakness of the south-east trades circulation.

(b) Diversion of an unusually large proportion of the south-east trades to South-East or East Africa during the monsoon period.

(c) Larger diversion than usual of the monsoon currents to Burma or Abyssinia.

(d) Very unequal distribution in India itself, due to local conditions established during the antecedent hot weather.

These factors are given in the probable order of their importance.

The importance of the work of seasonal forecasting in India may be judged from the following remarks :—

India is almost exclusively an agricultural country, with a population of nearly 300 millions. The material prosperity of practically the whole people is determined by the amount and distribution of the periodic rains. The variations in the amount and period of the rains are occasionally so great as to produce the most disastrous results in the staple crops over large areas. In 1899, for example, the crops failed more or less completely over an area several times the extent of England.

There is probably no country where the meteorological problems, of which these rainfall variations form one feature, are of greater interest or more practical importance. The daily weather and rainfall reports are studied during the greater part of the year with the closest attention by the officials, from the Viceroy downwards.

The Government is hence keenly interested in meteorological observation and investigation, and is most anxious to improve its meteorological service and utilise it for practical purposes, of which seasonal forecasting is one of the most important. To give two examples. A reassuring forecast at a critical period, followed by its realisation, might be of the greatest value to the agricultural population of a large province, as well as to the local and Imperial Governments. Again, a statement or forecast the probability of which was, say, at least 10 to 1 that the rains would fail more or less completely during a season over a large area might enable the Government to carry out early prudential measures for relief in the most economical and effective manner with the means at its disposal. The preparation and issue of seasonal forecasts will hence, I am confident, be in the future, as in the past, one of the most important duties of the Indian Meteorological Department.

There are several points in connection with weather forecasting in India which it is desirable should be borne in mind. The first is that weather in India is distinguished rather by the massiveness, intensity, and persistence of abnormal features than by the frequency and rapid succession of important weather changes. It is chiefly on this account that daily weather forecasts, even if they could be communicated with the necessary rapidity, are of no value to the Indian agricultural population. Also, the empirical knowledge of the significance of the important variations as factors determining or indicating future weather accumulates much more slowly than in Europe, and it is hence doubly important that in India the empirical knowledge derived from very limited experience should be, so far as possible, regulated and controlled by theory and scientific knowledge. It should also be remembered that there are large differences between the meteorology of tropical and temperate regions, and also between the relation of crops to weather in India and England. The instincts, habits, beliefs, education of the body of the people in

England and India also differ very widely. Hence the possibilities of the practical applications of meteorological science in India cannot be judged from the European standard, and may from that standpoint be unique.

The possibilities of usefulness of the work of seasonal or long-period forecasting in India are almost unlimited. To be acceptable and useful to the agricultural population of areas liable to drought, they should be fairly accurate with respect to the dates of commencement and termination of the periodic rains, their general character, and the probable occurrence of prolonged breaks likely to be injurious to the chief food crops. If the forecasts were found to be fairly reliable in these respects, it is quite certain that the agricultural population would value them and use them. Indications of a growing belief in the utility and value of this feature of the work of the Department by the people in different parts of India are not wanting.

The Government of India have sanctioned large changes in its Meteorological Department in order to enable it to carry out the extensions of work that recent experience has shown to be desirable. The Department is kept in touch with scientific opinion and judgment at home through the Observatories Committee of the Royal Society. The relations to other scientific departments in India are maintained by a special committee termed the Board of Scientific Advice. The scientific staff has been largely increased. The solar physics observatory at Kodaikanal and the magnetic observatory at Bombay have been placed under the Meteorological Department with a view to the complete co-ordination of the departments of scientific investigation for which they are maintained. Observational data for the whole Indo-Oceanic area are now being collected and tabulated with a view to the early publication of daily and monthly weather reports and charts of that area.

The objects of this last extension have already been indicated. It will afford the Indian meteorologists the data necessary for the investigation of the extension and intensity of the more important variations in the meteorology of the whole region, to correlate the abnormal features in the atmospheric circulation over the area, and more especially to ascertain the causes of the occasional failure of the monsoon rains in India. Finally, it will, it is hoped, enable the Department to collect the information and acquire the additional experience necessary in order to render the seasonal forecasts more reliable and satisfactory than they have been during the past six or seven years.

The area to be dealt with (viz., the Indo-Oceanic area) is partially covered by a number of independent meteorological systems, including those of Egypt, East, Central and South Africa, Ceylon, Mauritius, the Straits Settlements, and Australia. Large areas, as, for example, Arabia, Persia, Afghanistan, Thibet, and the greater number of the islands of the Indian Ocean, are now almost completely unrepresented.

The departments controlling these systems work independently of each other, chiefly for local objects, and are in no way officially correlated or affiliated. Their methods of observation and of discussion and publication of meteorological data differ largely. It is hence difficult, if not almost impossible, to make satisfactory comparisons of the data, and trace out for the work of current meteorology the extension or field of similar variations, their relations to each other, and their probable influence on the future weather.

The work which should be carried out in order that the investigation of the meteorology of the Indo-Oceanic area might be effective and as complete as possible includes the following :—

(1) The extension of the field of observation by the establishment of observatories in unrepresented areas, and the systematic collection of marine meteorological data for the oceanic area.

(2) The collection and tabulation of the data necessary to give an adequate view of the larger abnormal features of the meteorology of the whole area.

(3) The direction by some authoritative body of the registration, collection, and tabulation of observations by similar methods in order to furnish strictly comparable data for discussion.

(4) The preparation of summaries of data required as preliminary to the work of discussion, and for the information of the officers controlling the work of observation in the contributory areas. The earliest publication of the data should be regarded as essential for the use of officers issuing seasonal forecasts.

(5) The scientific discussion of all the larger abnormal features in any considerable part of the area and their correlation to corresponding or compensatory variations in the remainder of the area by a central office furnished with an adequate staff.

(6) Possibly, sufficient authority on the part of the central office to initiate special observations required for the elucidation of special features for which there are no arrangements in the general work of the various systems.

The Indian Meteorological Department is making preparations to carry out a portion of this work ; and will undoubtedly do the best it can single-handed with its limited means. It cannot do the work fully and as it ought to be done. It can do nothing which requires authoritative control over the remaining meteorological systems in the Indo-Oceanic field. It is collecting information from those who are willing to supply it and will utilise it for its special purposes.

It is evident the work can only be carried out fully by the co-operation of the various systems subject to limited control by a central office with acknowledged imperial or general authority behind it. The most important part of the work from the standpoint of the science of meteorology is the comparison and discussion of the whole body of observations. The constitution, position,

and authority of the central office is hence of the greatest importance. It is quite certain that none of the meteorological systems directly concerned can provide such a central office. If the work is to be carried out fully and systematically it can only be arranged for in England, and by the English Government assuming the general direction and control. At the present time a section of the English Meteorological Office is devoted to the study of oceanic meteorology for the information of mariners. Another section should be created for the study of imperial meteorology for the benefit of its dependencies and colonies. I have reason to believe that the Government of India would contribute its share towards the cost of this extension of work.

In the preceding remarks are given the chief reasons for an important extension of work now in progress in the Indian Meteorological Department, an extension which can only be carried out imperfectly by that Department, but which could be performed with most valuable scientific results by the co-ordination of the labours of the weather bureaus concerned, with a central institution or investigating office in England under Government control.

Perhaps I may be permitted, from my Indian experience, to add some general remarks bearing on the methods and progress of meteorological inquiry.

In India, the collection and publication of accurate current data relating to rainfall and temperature is required for the information of Government in its various Departments. The collection and examination of pressure and wind data by a central office with a view to the issue of storm and flood warnings is equally necessary. This work may, perhaps, be described as pertaining to descriptive or economic meteorology.

Economic meteorology, so long as it deals only with actual facts of observation, is not a science. Forecasts belong to the same department or branch of meteorology. They may be based on scientific theory and be obtained by scientific methods or the utilisation of empirical knowledge. The latter method is probably sufficient for by far the greater part of short-period forecast work, but the final development of that work and the preparation of long-period forecasts require the application of exact scientific methods and knowledge. And it is, perhaps, not too much to say that the extension of the range or period of forecasts is a measure of the progress of meteorology as a science. India, by the simplicity and massiveness of its meteorological changes (and perhaps Australia and Africa), appears to be best suited for the earliest experiments in this work.

India is, however, poor, not only in material wealth and capital as compared with England, but also in the appliances and means of scientific investigation, and hence looks to England for assistance and guidance in scientific matters. Unfortunately, England lags behind, not only the United States and Germany, but even behind

India, in the important field of scientific meteorological inquiry. It will suffice to give a single illustration of the anomalous and inferior position which England takes in such matters.

All meteorologists and scientific men generally are agreed that the exploration of the middle and upper atmosphere by any available means—*e.g.*, kites, balloons, &c.—is of the utmost importance at the present stage of meteorological inquiry. The United States, France, and Germany have taken up the work vigorously. The English Meteorological Office is unable, for want of funds, to share or take any part in the work. The force of scientific and public opinion is apparently powerless to move the English Government to grant an extra five hundred pounds annually for this work. The English Government, on the other hand, some time ago suggested that the Indian Meteorological Department should assist. The Government of India, recognising the importance of the work, has provided the funds and sanctioned the arrangements necessary in order that its Meteorological Department may march with the most progressive nations in this investigation.

India has no body of voluntary observers or independent scientific workers and investigators. Whatever is required to be done to extend practical and theoretical meteorology can only be effected by the Government Department to which that work is assigned, with the sanction and at the cost of the Government—which naturally considers chiefly its practical wants in relation to its limited resources. It is, from one point of view, a painful if not quite an unexpected experience to me, on my retirement, to find that the Government of India is, in its attitude towards meteorological inquiry, more advanced, more liberal and far-sighted than the English Government, and that England has not yet taken up seriously the work of scientific meteorological investigation. There are undoubtedly too many observations and too little serious discussion of observations. The time has arrived when investigation should go hand in hand with accurate observation, and should direct and suggest the work of observation, and also that the sciences directly related to meteorology should be considered concurrently with it. There are undoubtedly definite relations between certain classes of solar phenomena and phenomena of terrestrial magnetism. The probability of definite relations between solar and terrestrial meteorological phenomena is also generally admitted.

Data for the determination of these relations are being rapidly accumulated, and numerous problems connected therewith are waiting and ripe for investigation. They are too large and complex to be undertaken by present English methods, and can only be attacked by a body of trained investigators under arrangements securing the continuity of method and thought requisite for the prolonged systematic inquiry gradually leading up to their complete solution.

It would hence be desirable to enlarge the scope of the central

institution I have suggested, so as to include in its field of labour the investigation of the relation between solar and terrestrial meteorology and magnetism, so far as they can be solved by the comparison of the observations of the British Empire.

The central institution would thus have large and definite fields of work and most interesting problems for investigation. It would hence contribute towards the formation of a body of scientific meteorological investigators adequate to the importance and wants of the empire, and be of the highest educational as well as scientific value.

My predecessor in this position, Dr. Shaw, the head of the English Meteorological Office, made some remarks in his Address last year which deserve repetition in connection with this idea. He said: "The British Empire stands to gain more by scientific knowledge, and to lose more by unscientific knowledge, of the matter than any other country. It should from its position be the most important agency for promoting the advance of meteorological science, in the first place because it possesses such admirable varying fields of observation, and in the second place because with due encouragement British intellect may achieve as fruitful results in this as in other fields of investigation."

The establishment of the central institution as suggested above would provide a remedy for the defects pointed out by Dr. Shaw. The reorganisation of the English Meteorological Office is, I believe, under consideration. Is it too much to hope that a strong expression of opinion on the part of the British Association, and the influence of the learned University at which its present meeting is held, would induce the English Government to spend an additional 5000*l.* or 10,000*l.* annually for the promotion of meteorological investigation and the establishment of a central imperial institution in London in connection with its Meteorological Office?

Ancient and Modern Ideas about the Milky Way.

[Continued from p. 309.]

Thus the fine edifice so laboriously wrought seemed to crumble away in all directions. But several of Struve's criticisms found a resolute and well-armed adversary in Sir John Herschel, the son of Sir William. Inheritor of the paternal instruments and methods, he lived for several years under the beautiful sky of the Cape of Good Hope, and extended to the southern hemisphere the system of gauging and sounding. He showed that towards the South Pole, as well as towards the North Pole of the Milky Way, there is a manifest thinning of faint stars, so that the thickness of the system in this direction cannot exceed the mean distance of stars of the tenth magnitude. He found that one meets with fields everywhere, even in the median plane of the Milky Way,

absolutely empty, where the strongest telescopes show nothing but the faintest stars. A fact equally curious and significant is the existence of large fields formed of stars of mean magnitude, with an absence of the brighter stars as well as of the fainter. It seems evident that we have here before us not a dense and innumerable crowd, but a network of no great thickness and of loose texture, across which the eye passes quite freely. If the Milky Way in all directions was practically limited by the penetrating power of the eye or of the telescope, why should it not present to us an aspect much more uniform?

How can we explain the holes, the branchings, and the rents? The coal-sack, for example, becomes, on Struve's theory, a straight tunnel directed towards us, and practically in a mass of an incalculable thickness. Such an arrangement has many probabilities against it; but as blank spots of this kind are not rare in rich regions, it is by hundreds that we must count the tunnels thus oriented towards the observer. The appearance of the Milky Way obliges us, then, to regard it as a real agglomeration, of which the thickness is not infinite, but, on the contrary, small in comparison with the distance which separates us from it.

John Herschel has also the credit of confirming and determining some very important particulars previously foreseen by his father concerning the distribution of stars and nebulae.

The bulk of the stars are distributed, from the point of view of the Herschels, into two distinct classes. Some form well-defined globular masses much richer in the centre than at the boundaries. These are not very numerous, and no very precise law can be enunciated as to their distribution. Other stellar masses are irregular in form without any condensation towards the centre. These are in an enormous majority in the Milky Way, and it is they which contribute most to make it visible to the naked eye. An area of any extent taken nearly at random in the Milky Way contains a hundred times more stars grouped in clusters than an equivalent area taken elsewhere. One single region outside the Milky Way is richer in stellar clusters than the average. It is near the South Pole and forms the two Magellanic Clouds. In both these clouds stellar swarms and large and brilliant nebulae abound.

We owe to the two Herschels some remarks not less suggestive as to the nebulae. These are divided according to their appearance into three classes—small condensed masses, in the shape of a globe or of a ring; double spirals emanating from a central focus; lastly, extensive spots without a geometric contour. This last class is incomparably the most numerous and the most ancient known. It is the only one which was studied statistically by Sir William and Sir John Herschel.

If one sets down on a celestial map the observed positions of nebulae, it will be seen that the planetary or those of annular shape fall nearly always upon the Milky Way. The spirals fall

indifferently at all distances from its plane; the irregular nebulae have a decided antipathy for the Galaxy. Every region of some square degrees where the number of nebulae are in excess of the average is situated far from the Milky Way; the only exception is in the Magellanic Clouds. If we look in our hemisphere for the centre of the grouping of the nebulae, we find it precisely at the North Pole of the Galaxy. The scarcity of nebulae in the Galactic plane is quite as manifest in the opposite hemisphere, and if their preference for the pole is less evident this may be ascribed simply to the fact that this region is still imperfectly explored from the point of view of nebular research*.

We are thus led to the following conclusions: the planetary or annular nebulae form an integral part of the Galactic system; the nebulae in spirals are foreign to it; the vague and irregular nebulae are outside the plane of the Milky Way, but, nevertheless, under its dependence, as if this plane was the seat of a force capable of resolving the nebulae into stars or of keeping them at a distance.

Sir John Herschel indicated, with much reserve however, the theoretical consequences that the classification of stellar clusters and nebulae suggested. Without doubt the failure of the first attempt of his father to assign an exterior form to the stellar world rendered him circumspect; but without quitting the region of fact John Herschel had good evidence for an interesting conclusion. He recognized that the propensity to congregate in the Galactic plane, so marked for faint stars, did not exist for stars visible to the naked eye. The chart of stars of the sixth magnitude and brighter does not indicate the general line of the Milky Way, still less its branchings and doublings and lacunes. The bright stars admit of a plane of condensation and of symmetry, but this plane is inclined 20° to the Galactic circle and crosses in our hemisphere the constellations of Orion, Taurus, Perseus, Cassiopeia, Cepheus, Cygnus, and Lyra.

The late Dr. Gould, when he was appointed in 1870 to Cordoba as Director of an observatory without instruments, used his enforced leisure, as everyone knows, in making a naked-eye review of the southern sky. He rediscovered and completed the results of Sir John Herschel, prolonged the circle of bright stars across the constellations of Canis Major, Columba, Navis Argo, Crux, Centaurus, Lupus, and Scorpio. Similarly to the Milky Way, this circle is doubled along a part of its course, and the likeness becomes more pronounced if one takes only the stars of fourth magnitude and brighter. There is reason, then, to consider the Sun as forming part of a group of at least five hundred stars analogous to the group of the Pleiades and situated not far from the galactic plane. Our place in this group would be away from

* J. Herschel: 'Outlines of Astronomy' and 'Cape Observations.' These results are confirmed by the most recent researches of Stratonoff: 'Études sur la constitution de l'Univers,' 1^{re} partie, 1900, et 2^e partie, 1901.

the constellations Ophiuchus and Hercules, for in that region the trace of the mean circle of bright stars becomes indistinct*.

According to Mr. J. R. Sutton †, there is between the two circles a physical relation, for in the neighbourhood of their intersection the Milky Way presents an aspect disconnected and torn; several of its branchings converge upon the bright stars. The coincidence is perhaps curious, but it is certainly too difficult to admit that stars of the first magnitude may be at the same mean distance as the Milky Way in which faint stars so evidently predominate.

We shall feel upon more solid ground if we call to our aid the methods of spectrum analysis. It is well known that if we decompose the light of the stars by the help of a prism or grating, the spectra obtained are of very different character. One is thus led to separate the stars into categories independent of their situation or of their apparent magnitude. A first class (bluish-white stars) have a continuous spectrum crossed by hydrogen lines. A second class (yellowish-white stars) show numerous metallic lines, the same generally which are met with in the spectrum of the Sun. These two classes lend themselves to subdivisions upon which we will not now dwell. The following categories—red stars, stars with spectra formed of bright lines—are relatively less numerous, less luminous, and it would be premature to formulate general conclusions on the subject of their distribution.

Let us confine ourselves solely to the two first classes, the richest and the best known. It may be taken as established that the yellow stars are at a temperature less high than the blue, that they possess less actinic power for the same visual light, and that they include a large proportion of stars of large proper motion. It becomes thus very probable that the yellow stars form a relatively limited group, of which the Sun forms part, and that their luminosity, in general very great, is due more to their nearness than to their intrinsic light.

This hypothesis will be confirmed if we examine the distribution of the two classes of stars. The first class is obviously concentrated in the Milky Way, and it may be believed, as we shall see presently, that this concentration would be more marked if the investigation of spectra could have included very faint stars. But the yellow stars do not show this tendency in any degree, and they are collected rather about the North Pole of the terrestrial equator. We are thus led, following the idea put forth a century and a half ago by Lambert, to consider ourselves as citizens of a quite well-defined group much more extensive than the solar system, but much less so than the Milky Way, and stretching out in some direction different to that of the latter.

The researches that we have just described have received a considerable impulse at the Harvard College Observatory con-

* 'Uranometria Argentina.'

† "On the Plan of Sidereal Structure" (*Knowledge*, 1891, July 1).

sequent upon the progress in sensibility of photographic plates. It has become possible, by the help of a prism set up before the object-glass of a telescope, to register at one operation upon the same plate, in a few minutes, several hundreds of stellar spectra. The last catalogue published by Prof. Pickering comprised 10,351 items. Nevertheless it still remains to extend this chemical classification as far as the South Pole or to complete it in the northern hemisphere for the faint stars.

The visual classification according to apparent brightness is, on the contrary, complete for the entire sky as far as a limit of magnitude between 9 and 10. It includes nearly a million objects enumerated in six or seven volumes, with the data necessary for finding them. This work in the northern hemisphere demanded the efforts of a band of astronomers for twenty years. Dr. Gill, Director of the Cape of Good Hope Observatory, by using photography, put the southern hemisphere in a few years on the level of the other. Indeed he made a tangible advance, for he went a little beyond the limit of the tenth magnitude. This inequality between the two hemispheres will be repaired by the publication of the great international Catalogue now in course of execution, and which will comprise three millions of stars scattered over the whole of the celestial vault.

It is much quicker to count objects in a printed catalogue than the stars in the sky. To class figures written about them is a hundred times more expeditious than to estimate their magnitudes, making at the same time the notes necessary to prevent confusion. The publication of the Cape Durchmusterung suggested, therefore, the idea of re-making the soundings of Herschel, extended this time to the entire sky, and subdivided according to order of brightness. This work has been accomplished lately with as much perseverance as success by M. Stratonoff, an astronomer of the Observatory of Tachkent. The maps which embody this research are very instructive. They show the celestial vault divided by meridians and parallels into 1800 parts of nearly equal area. In each area a figure is inscribed proportional to the number of stars of a certain magnitude which are seen there. As many of these charts were made for each hemisphere as the number of subdivisions of the orders of magnitude.

The first chart includes the stars visible to the naked eye. We are at once met with the fact, already pointed out by John Herschel and Dr. Gould, that the bright stars are not at all concentrated in the Milky Way, but the charts which follow bring to light a fact which we did not know previously, that the concentration is not any more marked for stars of the magnitudes which follow, viz. 6.1 to 6.5 and 6.6 to 7.0. The intersections of the galactic plane with the equator, and generally the whole equator, are in both cases regions poorer than the average. The distributions are quite unlike in the two hemispheres. There is, then, cause to think that the Sun is included in a group more extended than that which

Gould supposed, not flattened towards the galactic plane, and extended in a direction nearly perpendicular to the terrestrial equator. Our position in the interior of this group would not be central.

It is only when we go below the magnitude 7.5 that the condensation becomes pronounced, and that the galactic plane more than crosses the rich regions. Chart 6 of the northern hemisphere (magnitudes 8.1–8.5) shows us the beginning of an agglomeration upon the equator about 7^h R.A., at the actual place where, up to now, a deficiency had been noted. In the chart following this (magnitudes 8.6–9.0) we see this agglomeration developing and becoming preponderant. This disappears in the following chart (magnitudes 9.1–9.5). Putting the charts side by side we are thus able to see the birth, the evolution, and disappearance of several distinct condensations. This points to the possibility of a relation between the light of a star and its distance, and that the process of sounding by enumeration of stars by magnitudes is not illusory. The plan followed by M. Stratonoff is in effect that of the naturalists who study an organic tissue under the microscope by successive cuttings.

The faintest magnitudes completely represented in the catalogues are 9.5 for the northern hemisphere, 10.0 for the southern. We are far from the old limit reached by Herschel, who was able under favourable conditions to count stars of the fourteenth magnitude. The last maps which it has been possible to prepare show better than all the preceding the agglomeration of stars along the galactic plane. But it must be carefully remarked that they do not reproduce in any degree the peculiarities of structure of the Milky Way such as we observe with the naked eye—the increases of brightness, the branchings, the empty spaces. The Coal Sack of the Southern Cross and the area comprised between the two branches which are separated from α Centauri to α Cygni, are regions quite as rich as their neighbours when we do not go beyond the tenth magnitude.

Thus the appearance of the Milky Way as we know it results simply from the presence of an immense number of very small stars, which cannot be separated by simple inspection nor even by many astronomical telescopes, but only seen as isolated bodies in instruments like that of Herschel.

The soundings of M. Stratonoff are, then, far from reaching the limits of the visible Universe, and it is to be hoped that they may be extended, but they do not the less deserve on this account the greatest attention. The comparison of charts makes it clear that there exists a number of stellar clusters all situated nearly on the galactic plane, some touching and penetrating it, others being separated by large spaces relatively empty. It is only for the clusters nearest us that it is possible actually to determine the mean distance, the figure, and thickness.

[To be continued.]

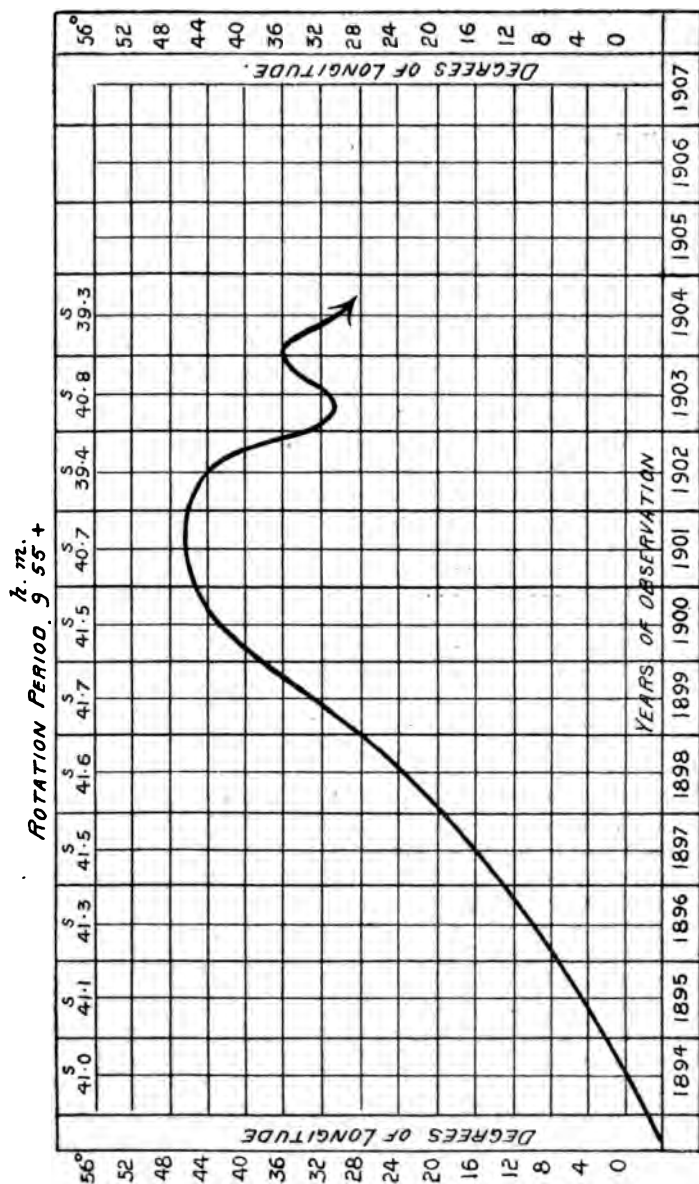
Variable Motion of the Great Red Spot on Jupiter.

THE rotation-period of the red spot on Jupiter has exhibited a singular variation during the last few years. Between 1877 and 1900 its velocity appears to have decreased without intermission, but in 1900 there was an evident increase, and in 1901 a notable acceleration of speed. This was continued in 1902, and up to May 1903, when a decided acceleration set in and endured until February 1904. Jupiter having then drawn near to the Sun, further observations could not be made until June 1904, when it was observed that the spot had undergone a marked increase of motion during the interval, the longitude being about 30° , whereas it was about 36° five months previously. For purposes of comparison it may be useful to give a table of the longitudes (System II., rate $9^{\text{h}} 55^{\text{m}} 40^{\text{s}}.63$) and mean rotation-periods during the last 10 years:—

Year.	Long.	Rotation.		
		h	m	s
1894	0	9	55	41.0
1895	5	9	55	41.1
1896	10	9	55	41.3
1897	16	9	55	41.5
1898	23	9	55	41.6
1899	33	9	55	41.7
1900	42	9	55	41.5
1901	46	9	55	40.7
1902	44	9	55	39.4
1903	33	9	55	40.8
1904 (to August)....	32	9	55	39.3

Whether the motion of the spot, in and since 1900, has been disturbed by the existence of a large dark mass in the S. tropical zone remains to be proved. The dark mass alluded to moves faster than the red spot, and rotates in about 22 seconds less time. The two objects being in the same current it has been suggested that the S. temperate spot *, before overtaking the red spot and while in conjunction with it, may sensibly accelerate its movement. It appears certain that the increase of velocity in the red spot has occurred at about the periods referred to. If the motion of the red spot is really influenced by the more rapid rate of the S. temperate spot, then the rate of the former would be slower when the two objects were in opposition, and swifter at the times of conjunction. This appears to have been confirmed by observation during the last $3\frac{1}{2}$ years. The point is an interesting one, but

* The spot connects the S. temperate belt and the S. side of the S. equatorial belt, and is sometimes called a tropical spot, but I have termed it "temperate" as it partakes of the motion of the S. temperate current.



Longitude and Rotation-period of the Great Red Spot on Jupiter, 10 years 1894-1904.

many further observations during the next few years are required to satisfactorily settle it.

The two spots were in conjunction late in 1902 July, early in 1904 June, and will occupy the same positions in the spring of 1906. The S. temperate spot has been well observed since 1901 May, and exhibits no sign of approaching disappearance; in passing the red spot it drifts southwards, flows along the S. side of that object, and then runs northwards and again occupies the S. tropical zone.

Its mean rotation-period during three years has been

$$9^h 55^m 18^{\cdot}7,$$

and it appears to complete one revolution of Jupiter relatively to the red spot in about 670 days, but the oscillatory motion of the latter introduces variations into the period.

Bishopston, Bristol,
1904, Aug. 20.

W. F. DENNING.

South Temperate Spots on Jupiter.

For a number of years, and probably from the earliest times, there have been brilliant white spots indenting the S. edge of the S. temperate belt of the planet. These features move a little quicker than the great red spot, their average time of rotation being about $9^h 55^m 18^{\cdot}7$, or 22 seconds less than the period of that object during the last 15 years or so. The permanency of some of these individual markings may afford a parallel to that of the red spot and hollow in the S. equatorial belt. It hardly seems likely that fresh spots similar in position and character break out in the S. temperate region every year. Little effort appears to have been made to identify the individual markings from one opposition to another. They are, however, recognized whenever Jupiter is favourably presented and their rotation-periods exhibit a remarkable constancy (always approximating $9^h 55^m 19^s$). In this respect they differ materially from certain other features on the disc.

On August 9 I saw two brilliant white spots encroaching on the S. side of the S. temperate belt. Their longitudes were $254^{\circ}1$ and $290^{\circ}1$. There is little doubt that these are the identical markings which I saw several times in 1903 and in preceding years. They are probably also the same as the two S. temperate spots Nos. 10 and 12 mentioned by the Rev. T. E. R. Phillips in 'Monthly Notices,' lx. p. 221. The following are some of my observations, which are not all closely accordant, bad definition and possibly alterations of shape or position in the objects having affected the results :—

I.			II.		
	Transit Time.	Long. System II.		Transit Time.	Long. System II.
	h m	°		h m	°
1899 June 5...	9 25½	189°1	1901 June 27...	12 17	190°8
29...	8 52	174°4	1902 July 16...	14 10	327°1
July 4...	7 58	172°9	Aug. 2...	12 53	317°5
16...	7 45	166°6	17...	10 11	315°5
28...	7 35	162°1	24...	10 50	311°5
Aug. 9...	7 18	152°9	Oct. 2...	7 18	284°2
1903 June 3...	15 55	137°3	14...	6 58	274°1
20...	14 46	129°9	Nov. 7...	6 31	260°7
22...	16 22	128°5	1903 June 20...	15 38	161°3
July 7...	13 34	121°4	July 7...	14 22	150°4
24...	12 10	106°4	24...	13 3	138°4
26...	13 42	102°7	Aug. 5...	12 49	134°4
Aug. 12...	12 28	94°4	7...	14 24	132°6
Sept. 3...	10 24	88°3	10...	11 52	131°9
17...	11 49	85°3	15...	10 52	127°6
Dec. 4...	5 1	38°4	Sept. 3...	11 18	121°0
1904 Aug. 9...	12 48	254°5	6...	8 43	118°5
			13...	9 14	110°0
			Oct. 12...	7 51	100°3
			17...	6 51	95°5
			Nov. 15...	5 25	80°1
			1904 Aug. 9...	13 47	290°1

The spots lose about $16^{\circ}4$ per month relatively to Crommelin's System II. (period $9^h 55^m 40^s.63$), so that their positions during ensuing months will be as under:—

Date.	I. Long.	II. Long.
1904 September 15	234°8	270°4
October 15	218°4	254°0
November 15	202°0	237°6
December 15	185°6	221°2
1905 January 15	169°2	204°8
February 15	152°8	188°4

I shall be glad to hear of any new or old observations of these objects. The past few years, with Jupiter in a low southern position, studies of markings of this character have been rather difficult, but now that the planet is taking up an increasingly favourable place, it is hoped that more attention will be given to the south temperate spots.

Bishopston, Bristol,
1904, Aug. 15.

W. F. DENNING.

CORRESPONDENCE.

*To the Editors of 'The Observatory.'**Hipparchus and the Precession of the Equinoxes.*

GENTLEMEN,—

In vol. xi. p. 300 of the *Observatory* (for July 1888) I pointed out a singular error into which Narrien fell, in fancying that ancient astronomers made observations from caves in the ground; the mistake apparently arising from using a Latin translation of Strabo (the original word is σκοπή), and confounding *specula*, an observatory, with *specus* or *spelunca*, a cave. The mistake was noted by Sir G. Cornwall Lewis, in *Notes and Queries*, 2nd series, vol. xi. p. 426. Had Narrien consulted the French translation of Strabo (which, begun by the order of the Emperor Napoleon in 1805, was not completed until 1819) he would have found the word σκοπή properly rendered “une espèce d'observatoire.”

I was reminded of this recently whilst reading Sir Henry Howorth's very interesting work ‘The Glacial Nightmare and the Flood,’ in which, speaking of the discovery of the precession of the equinoxes, he says (p. 370) that Hipparchus, “when observing, as was the fashion among ancient astronomers, the passage of certain stars across the mouth of a long shaft or well at the spring equinox”; and it seems to me that he had been probably misled by a recollection of the above passage in Narrien relating to Eudoxus, and concluded that Hipparchus observed in a similar manner. That the latter was acquainted, in the latter part of his career, with the precession of the equinoxes, rests, of course, upon the statement of Ptolemy, who says, in the seventh book of the ‘Almagest,’ that Hipparchus concluded it from a comparison between his own observations of Spica and those made by Aristillus and Timochanis. But there is much uncertainty about the dates of the latter, and the estimate of Hipparchus was certainly much less exact than that supposed by Sir Henry Howorth. Delambre remarks, as well he may, upon the fact that Ptolemy could find no observations later than the time of Hipparchus to check his own determination, which, as is well known, is much less than the true value.

Yours faithfully,

Blackheath, 1904, July 16.

W. T. LYNN.

Erratum in the Nautical Almanac for 1904.

GENTLEMEN,—

With reference to the explanation given by the Superintendent of the *Nautical Almanac* of the mistake in the adopted place of θ° Tauri, it is to be noted that in the volume of Greenwich Observations for 1897 epoch 1900.0 is given not merely on the titlepage of the section, but also in the heading of every page and in the subheadings of the separate columns of Mean R.A. and

N.P.D., Precession, and Secular Variation. It was supposed, therefore, that there could be no ambiguity on this point.

The reduction to the epoch 1900.0, as being that of the next Greenwich combined Catalogue, was applied to facilitate the comparison of observations of the same star in the several annual catalogues, and to expedite the work of preparing the Greenwich Nine-Year Catalogue for 1900.0.

Fortunately the reappearance of θ^2 Tauri on July 9 was observed at Greenwich by Mr. Dyson, though it occurred 4^m before the time predicted in the *Nautical Almanac*.

Royal Observatory, Greenwich,
1904, Aug. 23.

Yours faithfully,
W. H. M. CHRISTIE.

GENTLEMEN,—

With reference to the *ἀπολογία* of the Superintendent of the *Nautical Almanac* in the August number, it seems strange that he should have selected the Greenwich Observations for 1897 as his sole authority for the place of θ Tauri, seeing that this place depends on a single observation. Did this fact also escape his notice? Verily, *Qui s'excuse s'accuse*.

1904, Aug. 24.

Yours faithfully,
T. G. W.

OBSERVATORIES.

PARIS.—M. Loewy's annual report for 1903, after deploring the sudden death of M. Prosper Henry, states that the great work of re-observing all Lalande's stars was definitely completed in 1899, and is being followed by the observation of reference-stars for some of the zones of the Astrographic Catalogue. The Eros parallax campaign has occupied much attention, and a further circular (No. 10) has been issued, containing about 300 photographic observations of Eros and about 5000 photographic observations of neighbouring stars. It is hoped to form a catalogue of 3000 stars near the track of the planet, which will, in addition to its present use, eventually serve to determine their Proper Motions.

The probable errors of measures of stars on the Eros plates are given as follows:—

	Prob. error.
Well-defined images, magnitude 11.0 or brighter ..	0".14
Magnitude 11.0 to 12.0	0".18
Magnitude 12.0 to 13.0 (faintest visible)	0".22

The 7th fascicule of the Lunar Atlas has been published. An 8th fascicule, consisting of enlargements of old photographs, will be published this year. The discussion of the photographs is stated to indicate the presence of a former atmosphere, which distributed the ashes forming the ray-systems; also the absence of

running water is shown by the undisturbed condition of these deposits.

A Lippmann meridian photographic instrument is in process of erection. The results will be awaited with interest. It is proposed to make a new determination of the constant of aberration by the method of comparing two distant stars by means of a wedge-shaped mirror in front of the object-glass. It is rather surprising to read of a plan for observing the stars down to the 9th magnitude within 10° of the North Pole, since this work has been accomplished at Greenwich in the last few years.

As regards routine observational work, meridian observations of Sun, Moon, planets, and stars have been continued as usual. With the equatorials, 3 comets, 19 minor planets, 180 nebulae, 11 occultations of stars have been observed. 34 Chart plates and 32 Catalogue plates were obtained. 35 Chart plates were reproduced by heliogravure.

Provisional elements have been calculated for 90 plates of the zone $+23^\circ$. The measurement of Astrographic plates has, however, been practically suspended till the Eros plates are measured. Some of the latter are measured in four different positions, others in two only (differing by 180°).

The recent determination of the Longitude Paris—Greenwich is alluded to, but no results are yet given.

Meteorological and Time-signal work have been continued as usual. Two new physical investigations by M. Nordmann, "Study of Solar N Rays" and "Study of the Ionization of Atmospheric Air," are briefly alluded to.

BIDSTON, LIVERPOOL.—Mr. W. E. Plummer's Report for 1903 is very similar to those of former years. No additions were made to the buildings or to the equipment of the Observatory. A certain number of transits were observed for determination of time for distribution of signals, and for this purpose the transit-instrument is said to be sufficiently accurate, but it still shows some unaccountable variations of azimuth. The positions of the comets of the year were observed with the equatorial, and the same instrument was used to measure the relative positions of double stars and the diameters of the brighter planets. The book in which these particulars are given also contains the meteorological observations made during the year, and also the records of the earth-tremors shown by a horizontal pendulum. There appear to have been 160 such during the year, all lasting a considerable time, half an hour perhaps in the mean.

PUBLICATIONS.

ASTRONOMICAL AND HISTORICAL CHRONOLOGY *.—This is a little book which apparently was intended to appear in time to take

* Longmans, 1904. 70 pp. 2s. net. By W. L. Jordan.

part in the controversy which raged a few years ago as to the precise moment when the twentieth century was to commence. Its publication having been postponed for a time, the author has taken advantage of the delay to amplify his contention that the new century began January 1, 1900, and to endeavour to show cause why chronologers generally should follow the example of astronomers and insert a year 0 between 1 B.C. and 1 A.D. He also goes into the vexed question of the Cycle of Meton, reviewing the different versions and explanations, with several tables, but the question of the zero year is the main point.

Now, though we freely admit that the year is a very important part of a date, yet it is not the only important part, and this Mr. Jordan admits, as he lays stress on the fact that different days have been used for the commencement of the year. In ordinary chronology, the year 1904 is the 1904th year of the birth of Christ (or rather the year that has been allowed to stand for it), that birth being conventionally agreed to have taken place close to the end of the year, although Chinese ideas would go back nine months earlier, following a custom not in general use in this country. To ordinary notions the birth represents a point of time and not a year at all, so that it is quite logical to call the year then practically ended the year before that point, 1 B.C., and the next 1 A.D. Like many other controversial points, it is simply a question of terminology. What is a birthday? Obviously if it be the day one is born, then nobody has more than one. But the feast that is celebrated is not on the real birthday, but on the anniversary, which is conventionally called the birthday. Hence it is quite logical to call the day one reaches the age of 21 the 21st birthday, though some people argue that it is the 22nd. In like manner Mr. Jordan will have it that the first number is 0, and the second is 1. This, however, is so contrary to philology, to say the least of it, that we cannot admit it. We do not deny that on one's conventional 21st birthday one enters one's 22nd year, but one also completes one's 21st year, the day itself belonging partly to one and partly to the other in most cases.

Sir John Herschel went so far as to admit a zero year to begin an era, but probably not for the reason given by Mr. Jordan, as witness his other suggestion that there should be a year 0 B.C. and a year 0 A.D. He must have been working on the analogy of zones of declination, whose numeration changes sign at the equator, which divides the zone $+0^\circ$ from the zone -0° . This is quite logical, though Mr. Jordan will not allow it. The crux of the matter is, as we hinted above, the definition of the year or the zone. As a matter of fact, the meaning of the zone $+0^\circ$ is the zone containing stars of declination 0° plus something less than a degree, and of the zone -0° , that containing stars of declination 0° minus something less than a degree; this necessitates the conclusion that 0° without $+$ or $-$ is not a zone at all, but is the equator itself, just as we maintain that the zero is not the first number, but the point from which the first number is measured.

So long as Mr. Jordan considers the birth of Christ to be a year, so long will he find the argument for a single zero year plausible. Sir John Herschel evidently did not so consider it.

As a matter of convenience, it would appear to be a good thing if everybody's dates agreed, but, as far as the past is concerned, no amount of alteration now can dispose of the difficulties, and for the future there are practically none to dispose of. The other point as to the true commencement of our era is a mere convention, apart from the point concerning cardinal and ordinal numbers, wherein we differ from Mr. Jordan.

Incidentally we may remind him that there is a very great probability that the birth of Christ took place not in B.C. 4 or B.C. 3, but in B.C. 7, or, as he might call it, B.C. 6.

The book does not, however, claim to be the last word on the subject, as witness the dedication to the librarians of Florence and Pisa (as representing the two sides of the controversy), with an appeal for further evidence from their archives. W. W. B.

NOTES.

COMET NOTES.—*Ast. Nach.* 3962 contains the following elements and ephemeris of Tempel's Second Periodic Comet, whose period is 5.28 years, by MM. Schulhof and Coniel. Perturbations by Jupiter and Saturn are included. The comet will be very faint, but should be observable in large instruments:—

Epoch and osculation 1904 Oct. 30.0 Paris M.T.

M		357° 51' 49"	} 1904.0.
ω		185 44 39	
Ω		120 59 52	
i		12 38 55	
ϕ		32 50 4	
μ		672".175	
log a		0.481683 *	

Ephemeris for Paris Midnight.

		R.A.			S. Dec.				R.A.			S. Dec.	
		h	m	s					h	m	s		
Sept. 2....	14	55	53		8	46	Sept. 30....	16	6	44		16	37
6....	15	4	51		9	55	Oct. 4....	16	18	24		17	38
10....	15	14	12		11	4	8....	16	30	27		18	37
14....	15	23	56		12	13	12....	16	42	53		19	33
18....	15	34	3		13	21	16....	16	55	41		20	26
22....	15	44	34		14	28	20....	17	8	50		21	15
26....	15	55	27		15	33	24....	17	22	19		22	0

Herr Palisa made an unsuccessful search both for this comet and Encke's early in August; the brightness of both is, however, increasing.

* Erroneously printed 0.418683 in *Ast. Nach.*

Comet 1904 I. was observed by Signor Cerulli on July 27. He states that it was readily visible with $15\frac{1}{2}$ inches, in spite of the Full Moon. Its place was in perfect agreement with Nijland's ephemeris, showing that the orbit is sensibly parabolic.

A. C. D. C.

MINOR PLANET NOTES. — The following new planets are announced:—

Planet.	Date.	Discoverer.	Place.	Mag.
OH	1903 April 28	Peters.	Washington.	..
OJ	June 29	"	"	..
OK	1904 July 18	Götz.	Heidelberg.	12.2
OL	Aug. 2	Wolf.	"	11.9
OM	2	"	"	11.5
ON	3	"	"	13.2
OO	4	"	"	13.0

Planet OG was independently discovered by Charlois on July 7.

Planets OH, OJ are probably identical with 353 Ruperto-Carola and 411.
A. C. D. C.

OBITUARY.—PROF. J. D. EVERETT. We regretfully record the death of Prof. J. D. Everett, F.R.S. It is now seven years since his retirement from the Chair of Natural Philosophy in Queen's College, Belfast. He is also widely known as the Translator of "Deschanel," and for his work on the C.G.S. system of units.

THE CENTENARY OF DOPPLER*. — We have received from Dr. Karl Haas a copy of the address that he read on the occasion of the celebration of the centenary of the birth of Christian Doppler. It is strange to reflect that Doppler, in 1842, years before Kirchhoff and Bunsen had demonstrated the possibilities of the spectroscope, first announced the principle bearing his name, which has since become so famous and so fruitful of application. He himself was not happy in his original suggestion, for he sought to employ the "principle" as a means of explaining the colour of double stars, and it may be that some of the controversy that at one time raged round this question originated in its faulty application rather than in the generic principle underlying the suggestion. So far as acoustical waves were concerned the matter could be, and was, settled by experiment, and Dr. Haas not only referred to the various tests that had been made to prove the validity of the "principle," but exhibited some of the apparatus that had been employed, and repeated the experiments. Dr. Haas briefly referred to Doppler's strenuous life and work in other departments of natural science, showing that the enunciation of the principle was not a matter of accident, but that it was led up to by philosophical study, and put before the audience the

* From *Nature*, 1904, July 28.

grounds on which Doppler was entitled to their respect. But the valuable work that had been accomplished in the realm of spectroscopy naturally made him linger on this part of the subject, and he gave a useful summary of what has been effected by those who saw how the principle could be applied in special directions.

A COMMITTEE ON SOLAR RESEARCH.—It will be gathered from other pages of this number that Prof. Turner is now in America. One reason for this is that he will attend as one of the delegates of the Royal Society, and also of the Royal Astronomical Society, a meeting to be held in September in connection with the International Congress of Science at the St. Louis Exposition. The National Academy of Sciences appointed a Committee on Solar Research, consisting of Profs. Hale, Campbell, Langley, Michelson, and Young. This Committee thought that the best results would be obtained by inviting committees of other societies and other individuals to act with them, and consequently invited the Royal Society and Royal Astronomical Society to form committees and send delegates to a General Meeting to be held at St. Louis. Both these English societies responded to the request and suggested that certain persons should go as delegates.

THE PERSEID METEORS.—Rather a good show of meteors from this radiant was observed at Greenwich this year. On the night of August 11, 275 meteors, probably not all Perseids, were seen in about seven hours. The total number observed on the four nights August 10-13 was about 470.

THE NINTH SATELLITE OF SATURN.—It may be remembered that in 1899 Mr. W. H. Pickering announced the discovery on some photographs of Saturn taken in 1898 August of a new satellite. The name Phœbe was given to this object, but little has been heard of it since. It appears, however, not to have passed into obscurity, for a note from Harvard promises a discussion of recent observations of this in a forthcoming volume of the Observatory 'Annals.'

ADMIRAL WILLIAM WHARTON, after a distinguished service of over 20 years as Hydrographer to the Admiralty, retired from the post on August 1. Previous to entering on this office he had acquired much experience in Surveys and Deep-sea Soundings; but perhaps his chief work was, when in command of H.M.S. 'Sylvia,' he determined fundamental longitudes in the Southern Hemisphere. During his long tenure of office he has had to advise on Arctic and Antarctic expeditions, investigations of coral-reefs, and many kindred subjects brought before the Board of Admiralty. In carrying out this work, as well as in the ordinary routine of his office, he was always actuated by a spirit of fairness, and, as a writer in the *World* says, "Quiet, unpretentious, and ever-accessible and just, the Admiral made many friends

and could have no enemy." More closely connected with astronomy was his help afforded in Solar Eclipse Expeditions. We trust he may long enjoy the leisure afforded by his retirement.

CAPTAIN MOSTYN FIELD, who succeeds Admiral Wharton as Hydrographer, is also a Survey officer. He is known to scientific circles from his perseverance in the investigations of Coral-Reefs. In connection with the borings on the Atoll of Funafuti, Professor Bonney writes:—"The most important contribution, however, is afforded by the investigations of Captain Field, who made a complete topographical survey of the Atoll, and a vast number of soundings both in the lagoon and the outer sea. He also carried out the magnetic and tidal observations." He has a long career of usefulness before him.

It is announced that Mr. H. C. Russell, Government Astronomer of New South Wales, is to retire at the end of the present year, after forty-six years' service.

AN International Astronomical Congress will begin its meetings at Lund on September 5.

FROM AN OXFORD NOTE-BOOK.

Niagara Falls, 1904, July 25.

It is eight years since I was at Niagara with Colonel Rivers, and the Clifton House, where we then stayed, is a mass of blackened ruins, a fire having destroyed it some few years ago. But the waterfall is still running, and it must be confessed that it is as impressive as ever. It is perhaps a little more difficult to avoid seeing the ghastly advertisements which disfigure the Canadian bank, especially the one which has been planted on the ruins of the Clifton House; but if one can remember to turn one's back on these before an involuntary glance has spoiled one's temper for the day, the mighty Falls will speak as eloquently as ever. We went in the little steamer which puffs up as near as it dare; and we noticed that the flags, which hung limply from the masts elsewhere, in the neighbourhood of the falls were blown straight outwards by the wind from the rushing cataract. If Bradley had been on that steamer instead of on his pleasure boat on the peaceful Thames when he watched the changes in direction of the wind-vane, his meditations would scarcely have taken the direction which led him to the discovery of aberration.

BEFORE reaching here we have seen many and wonderful things at the Harvard and Albany Observatories, some few of which I am about to try to put down on paper. I hope it is not too irreverent to use these few days in such a way—not too much like the conduct of those amiable people who have their photographs taken with the Falls as a background. The roar of the cataract does indeed make a noble background for reflections and recollections, and from our experiences of the last fortnight it

should be possible to make a selection not unworthy of such a setting. For, unless I am mistaken, we have had glimpses of what Astronomy is going to be in the future. The prospect is by no means complete as yet: we shall see more wonders as we go West; but already we have seen and heard enough to recognize that the chief additions to our knowledge of Astronomy in the near future will be made here in the United States. Perhaps we knew this more or less before, from what we have read; but it is much more impressive to see and hear the things and men one has read about.

ONE of the first pieces of news we heard on reaching Harvard was that Professor W. H. Pickering had succeeded in re-finding Phœbe, the ninth satellite of Saturn, which he discovered by photography some years ago. He has got thirty-five plates showing it—an object of extreme faintness, however, which even the most powerful telescope may fail to reveal to the eye. It gives some notion of the difficulty of the research that the discoverer spent an average of four hours on each of the thirty-five plates, though he, of course, knew the approximate position of the satellite on the majority of them. There is a remarkable feature about the orbit, which, however, I must not mention here, as the discoverer wishes to make the first announcement himself in a number of the *Harvard Annals* just passed for press. But I can certainly guarantee everyone who reads that number a genuine surprise, mathematicians especially.

BUT this particular achievement lies rather outside the main work of the Harvard Observatory, which is chiefly photometric and spectroscopic. More characteristic are the numerous discoveries of variable stars, near the Orion nebula and in the Magellanic clouds. More than a dozen years ago Dr. Isaac Roberts claimed to have discovered some twenty or thirty variables near the Orion nebula by comparison of a single pair of photographs, and I believe a few of those he specified are identical with some now independently discovered at Harvard; but the Harvard observers enumerate several hundreds of these objects, and they have a number of photographs to establish the variability. By these recent additions the total number of known variables has been more than doubled in a very short time, and may soon be doubled again. One or two cases are of particular interest: there are three stars so close together that they are only separated with difficulty on the enlargement, all varying in different periods; and again there is a variable so close to a tiny nebula that a hasty inspection might attribute variability to the nebula. Such cases suggest that in some cases where a star seems to vary in a complex manner, two or more periods being superposed, there may be two or more stars varying independently, but so close together that they cannot be separated.

ANOTHER characteristic piece of work is the classification of the spectra of variables by Mrs. Fleming. She has found a definite clue in the behaviour of the hydrogen lines; but I hesitate to speak from memory about a not very familiar line of work, except to say that the series of photographs presented a convincing appearance of sequence. Amid the numerous other calls on their attention, no time was found to determine whether this sequence had any relation to the independent sequence which seems to govern the form of the light-curves; but the comparison will be of interest. It seems clear from the relationship of variables to clusters, and to nebulae, and from these suggestive arrangements in series, that the study of variables is bound to play an important part in the elucidation of cosmogonies.

I HAVE often wondered whether some of our amateurs might not find it interesting to follow up some of the variables through their faint minima by photographic methods. A comparatively humble instrument would photograph them, if the exposure were long enough; and the work, though simple, would be of permanent value. It is, however, only fair to say that Professor Pickering, after working at photographic photometry for more than a dozen years, still finds it difficult to devise satisfactory methods for getting absolute magnitudes, though the "method of Argelande" (or rather of W. Herschel, who used it long before Argelande) can, of course, be applied to a photograph, and will give the magnitude of the variable in an unknown sequence of comparison stars. Some of these difficulties are being patiently unravelled by Mr. King, who is in general charge of the photography of Harvard. He has found, for instance, that there is a sensible difference in photographic action depending on the temperature at which the plate is exposed, and got an approximate measure of it. (This we shall have to take account of in measuring the brightness of the corona, for instance.) Mr. King has also carried on an extensive series of comparisons between different batches of plates; between members of the same batch at different ages, and so on; the general result of which is reassuring. I remember his curves rightly. There is a slight general increase in sensitiveness, the action being otherwise little affected, if we allow for natural accidental errors.

At the Harvard Observatory one may go from one such piece of work to another day after day; to deal with them all without wearying my readers is beyond my skill. There are piles of plates, representing individual researches, in odd corners, and piles of MSS. in others. There is a revolving table littered (do I use the word?) with piles of proof and MSS., at which the busy Director sits and literally revolves the various topics; and the flood of work pours along so fast that the shelf in his study, which I saw in 1896 partially filled with 'Harvard Annals,' and which he then hoped to fill before the end of his term of office.

is already completed; and the tide has swept round the corner into the next one. But even the sight of all these things scarcely produced on my mind the impression made by that of the actual plates themselves—the stores of thousands of plates in which each part of the sky is represented *a hundred times over*. There is the *history* of the sky for the last decade, and there it will accumulate for decades, nay centuries to come. For who will be bold enough, or strong enough, to stop that accumulation now it has been started, and what kind of reason can be given for doing so? The value of it has already been proved; and the future can only enhance that value and make it more and more imperative to add to the records. It is always risky to prophesy unless you know; but to day-dream is pleasant, and not without profit occasionally. What do we picture as the future of Astronomy in our day-dreams? How many such collections of photographs will there be in the world?

TALKING of prophesying, here is another proof that prophets continue to receive honours in an irregular manner. In July last, Oxford conferred an honorary degree upon W. D. Howells; but here, in his own country, I have just had the following experience. I have a vague recollection of a book of his called “*Their Wedding Journey*,” which opens, if I remember rightly, at Niagara; and I thought it might be pleasant to refresh one’s memory of it on the spot. But on enquiring at the leading book store for it, I was met by the response: “Howells? What sort of books does he write? Detective stories?” *Detective stories!* Poor William D.!

THE mention of one incongruity suggests another. Who would have expected that, when opportunity offered of spending an afternoon with Mr. Chandler, we should agree to spend it, not in astronomical discourse, but in seeing a base-ball game? But so it was, and most thoroughly we all enjoyed it. He got us seats right behind the catcher and told us all the points of the game; and a splendid game it was, though rather warm even for spectators. We read the base-ball news in the papers now and feel that we know all about it. But we had some talk on other matters, and I got one or two pretty problems from Mr. Chandler: he was careful to say they were not his own, and they may be better known to others than to me; but I will give them on the chance:—

(1) On the floor of a rectangular room, sides (say) 3 yards and 4 yards, a rectangular strip of carpet a yard wide is laid so as to have its four corners at the four walls of the room. What is the length of the carpet?

(2) A fly crawls on the walls of a room from one corner to that diagonally opposite. What is its shortest path?

(3) Finally, the “great clock problem” which is being set in one of the Boston papers:—On the morning of December 25,

1903, an old clock, which was a fine time-keeper, struck 11 while the hands were pointing to exactly 7 o'clock. Just as the clock struck 3 it stopped, having run down. It was wound up and started again at precisely 7.30 o'clock in the evening. The next time it struck 1 what hour was it, and was it time for the Sun to be up?

DURING our stay at Harvard I spent a thoroughly interesting morning at the Jefferson Physical Laboratory, where a number of researches are in progress. One investigator is measuring the deadening effects of mats and curtains on sound, and his tests are so delicate that his friends chaffingly declare he can tell when he has had his own hair cut. He tells an interesting story of a millionaire who came to him for advice in a somewhat trying case: being sensitive to sounds he had ordered a sound-proof room, sparing no expense. The architects had done their very best, with the result that the room constructed was extraordinarily sensitive to sound, so that the would-be recluse at the top of the house could hear anyone tap the walls in the basement! In another room we saw some beautiful examples of fluorescence; the exhibitor confessed, however, that as yet he had no clue to any general principle by which to group his results. Another investigator is photographing and measuring the lines of short wave-length recently discovered by Schumann. It seems doubtful whether these lines will ever interest astronomers, for our atmosphere absorbs them completely. More directly interesting were the experiments conducted by Professor Hall on the deviations of falling bodies from the vertical. Experiments hitherto have almost always given a *southerly* deviation, which theory cannot justify, as well as the easterly, which it can; and Professor Hall's work is no exception, although he has got the southerly deviation down to 0.005 cm. in a drop of 23 metres. The problem is a classical one, having been originally propounded by Newton; and Hooke was the first to claim, and to find, a southerly deviation.

IN the Jefferson laboratory there is a collection of old instruments, some of them astronomical. In particular there is a wonderful orrery, which took an ingenious mechanic ten years to build, and was purchased for the University by means of a lottery, for £550. Governor Boudoin got up the lottery for the purpose; and in view of this beneficent act, figures of Boudoin take turn with figures of Newton and Franklin in decorating the periphery of the machine. The constructor chose an unfortunate moment to begin his work; for after he had spent five years on it, Uranus was discovered; and he apparently found it impossible to alter his design so as to include this new planet. He has, however, thrown in an extra little Moon for our Earth; possibly as a sort of compensation for omission.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, F.R.A.S.,

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THE OBSERVATORY,

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OCTOBER, 1904.

No. 349.

The Meeting of the British Association at Cambridge.

ON Wednesday evening, August 17, the President of the Association and Prime Minister (Mr. A. J. Balfour) gave his address on "Reflections suggested by the New Theory of Matter" to an audience of more than 2000 persons (including Lord Kelvin, Lord Rayleigh, Lord Rosse, Lord Avebury, Sir W. Huggins, Sir A. Rücker, Sir J. Eliot, Bishop of Hereford, Sir W. Abney, Prof. Becquerel, Sir D. Gill, Major MacMahon, Sir N. Lockyer, Prof. Schuster, Prof. G. H. Darwin) in the Corn Exchange.

On Thursday, August 18, the sectional meetings began. The following is a brief *résumé* * of the proceedings in Section A (which is divided into subsections—Physics, Astronomy and Cosmical Physics, and Mathematics).

The address to the two branches was given by Prof. Horace Lamb mainly on Prof. Stokes's influence on the development of mathematical and physical sciences. Comparing the earlier and later school of mathematics, he said perhaps the most important feature was its increasing relation to, and association with, experiment. In the days when the chief applications of mathematics were to the problems of gravitational astronomy, the mathematician might well take his materials at second hand. Generations of Cambridge students recalled the enthusiasm which characterized Stokes's experimental demonstrations in optics. The practical character of the mathematical work of Stokes and his successors was shown especially in the constant effort to reduce the solution of a physical problem to a quantitative form. There were many striking examples of the fruitfulness of the combination of great mathematical powers, that the question might well be raised whether there was any longer a reason for maintaining a distinction between mathematical and experimental

* [We are indebted to Mr. Hinks and to Mr. F. A. Bellamy for this.—Eps.]

physics. It might be held that the highest physical research would demand in the future the possession of both kinds of faculty. Care must be taken, however, not to erect barriers which would exclude a Lagrange on the one side or a Faraday on the other.

The rest of the address related more to geometry. A vote of thanks was proposed and seconded by Mr. A. J. Balfour and Lord Kelvin.

Among the papers in the Physical Section that by Prof. W. Wien (of Würzburg), on experiments to decide whether the ether moves with the earth or not, was specially interesting to astronomers. The author described the theoretical views that had been put forth and the experimental work that had been done; he concluded by describing his own experiments, which was similar to Foucault's velocity of light experiment, but he determined that the ray of light from his two revolving mirrors should not return in the same path; by this method he hoped to detect a difference in the velocity of light when the observations were made with the ray of light going in the same direction as the earth or the reverse. He stated that he was unable to get any definite positive results, as the accuracy required did not reach that of the best determinations of the velocity of light. But the difficulties were far greater, because his mirrors had to rotate with the same velocity (at a great distance between them), though that was only necessary for a short time, and the angular velocity could be detected by a change in the direction of the ray: he had hopes that these difficulties would be overcome.

Other papers were "An Experimental Verification of Newton's Second Law," by W. D. Eggar; and "A Modification of Fitzgerald's Model of the Ether," by J. B. Burke.

The Subsection of Astronomy and Cosmical Physics has kept its cumbersome title unchanged for another year, after high debate between those who hold that Cosmical Physics includes Astronomy, and those who suggest that Astronomy proper is not Cosmical Physics in the ordinary sense of the word Physics, but, if anything else than Astronomy, is Applied Geometry! It met at Cambridge on Friday, August 19, under the chairmanship of Sir John Eliot, whose address on Indian Meteorology has already been published in the *Observatory*. Proceeding to Cosmical Physics first, it heard papers by Sir Norman Lockyer and Mr. Newall on Criteria of Stellar Temperatures, the first maintaining the view that the criterion is the extension of the spectrum into the ultra-violet; the second arguing that we can imagine so many things producing extension in the ultra-violet that we have no right to say that the effective cause is temperature. Profs. Lummer, Kayser, and Schuster took part in the discussion which followed, the latter urging that an attitude of negation is unsatisfactory, and that, remembering always that it is only a working hypothesis, we may as well agree for the present to call what causes the extension

into the ultra-violet temperature. Two papers on the Spectra of Sun-spots brought Father Cortie and Dr. Lockyer into a friendly divergence of opinion on the subject of "crossing points" and the broadening of "unknown" vanadium lines, and papers by Profs. Birkeland, Ångström, Teisserenc de Bort, and others prolonged the meeting well into the afternoon.

On the Monday Mr. Milne presented his usual interesting report on Seismological Investigations, after which Dr. Shaw, in the absence of Mr. Dines, presented the Kites Committee Report, dealing at length with the experiments carried out at Crinan this summer on a vessel lent by the Admiralty.

The meeting then turned to Astronomy, and Sir David Gill spoke of some of the problems which still present difficulties. Dealing with the co-ordination of big questions, he referred to the possibility that direct methods of finding the solar parallax would establish a result in flat contradiction to that required by the constant of aberration and the simple aberration theory, and would hand over to the mathematical physicist one of those problems in which he delights to revel. After referring to the work which is in progress, of finding the solar parallax from photographs of Eros taken at the last opposition, and commenting on the fact that we are faced with the evidence of hitherto unsuspected systematic errors when we combine the results of different observatories and telescopes, he remarked that the next favourable oppositions of Eros occur in 1931 and 1938, and *it is not too soon to begin active preparation for them!* He suggested that the path of the planet at those oppositions should be computed at once, that the region should be photographed, the star-places determined, and the results exhaustively discussed, so that next time we may employ no instruments or methods that are not of proved and reliable quality. He also insisted that the mass of the Moon is of equal importance to the solar parallax, and that the definitive discussion of the Eros work must be planned with a view to determining this constant also. Passing to the question of latitude variation and the aberration constant, he regretted that the International Latitude-Variation Scheme takes no account of the latter, expressed his belief that refraction is a function of the season, and that it will be necessary to develop the methods of determining the aberration constant from R.A. observations, and secure at the same time a determination of the horizontal component of the latitude variation. He concluded a striking address, which we hope will soon be published in full, by referring to the urgent need for reducing and publishing certain series of old observations, among them those of Dr. Rigaud, made at the Radcliffe Observatory*.

* [Sir David was in error in so far as he called the observations those of Rigaud. The Rev. S. P. Rigaud was the Savilian Professor of Astronomy and Radcliffe Observer at Oxford from 1827 to 1838, but he was a bookworm chiefly, as may be seen from his splendid 'Life of Bradley'. The meridian work certainly was continued during these years under his direction by Angel Lockey

In the discussion Prof. Backlund emphasized the importance of the old unreduced Oxford observations, Sir Robert Ball and Mr. Hinks acknowledged the references made to the Cambridge reductions of Eros photographs, and Dr. Russell remarked that by the time the oppositions of 1931 and 1938 came round, we should have another very strong determination of the solar parallax from the remarkably large inequality in the motion of Eros due to the Earth.

Dr. Shaw then brought up a report adopted by a Committee of the Council, recommending the adoption in British Meteorology of the Centigrade system reckoned from the absolute zero for thermometers, and a division of barometer scales having for unit *half* a degree of absolute pressure on the C.G.S. system. The discussion which followed showed some unanimity of opinion against the proposal. Capt. Creak, R.N., and Capt. Tizard, R.N., spoke of the dangers of introducing the system into the Navy; Dr. Chree thought there was no danger in taking for zero the freezing-point instead of the absolute zero; Mr. Buchanan delivered a panegyric on the life and works of Fahrenheit; Prof. Wind thought that the proposal should first be laid before the International Committee of Weights and Measures at Breteuil; and the President urged that, inasmuch as weather reports are furnished for the use of the Government and the public, it is useless to think of altering the meteorological units until the metric system has been generally adopted.

Dr. Russell contributed a valuable statistical paper on the masses of the stars, and came to the conclusion that the stars are much more nearly uniform in mass than in any other respect.

On the Wednesday the proceedings included the Reports of the Falmouth and Ben Nevis Observatories; a paper by Dr. Mill, to show that heavy rainfall is general on the left of the path of a cyclone; a paper by Prof. D'Arcy Thompson, with an ingenious elucidation of the well-known obscure passage in Plato describing a system of the planets; and a finely-illustrated account of the South Kensington spectroheliograph and its work by Dr. Lockyer.

On the Friday afternoon of the meeting the Cambridge Observatory was thrown open to the Members of the Association. The various instruments were shown, and in the library there was an exhibition of old and famous books; of eclipse photographs and star-spectrograms, by Mr. Newall; of solar-parallax photographs and reductions, by Mr. Hinks; of stellar-parallax photographs and results, by Mr. Russell; of the determination of the mass of

(who died in Oxford a few years ago) and others, but the unreduced and unpublished observations, numbering many thousands, of Sun, Moon, planets, and stars, were mostly made by or under the direction of Dr. Hornsby and Dr. Robertson, the two Radcliffe Observers from 1772 to 1828. These old observations of good accuracy are quite as important in astronomy as the most refined meridian observations of the present day; the old observations cannot be repeated, the new ones can be.—F. A. B.]

Jupiter from heliometer observations, and photographs and results of his floating photographic zenith-telescope, by Mr. Cookson; of diagrams showing the seasonal variation in the errors of the meridian circle, by Mr. Hartley; and records from the self-recording barograph and electrograph recently installed.

In the Section of Geography an important and interesting discussion on the methods of topographical survey was opened by Major Close, who referred to the large amount of geodetic work that had been accomplished in India and Africa, and the proposed formation of a geodetic arc from St. Lawrence to Vancouver. He thought that photography has a very limited usefulness in this work, but this opinion was not entirely shared by those who took part in the discussion.

On Tuesday afternoon Prof. J. H. Poynting gave an exceedingly interesting address on "Radiation in the Solar System." He said that a black surface emitted more energy than any other. From Newton's time efforts had been made to determine the relation between energy emitted and the temperature of the surface; these resulted in Stephen's law, according to which the radiation was proportional to the fourth power of the absolute temperature. The difficulties arising from atmosphere absorption were referred to. The result of applying Stephen's law to the solar surface was to give a temperature of 6250° absolute; for a small black sphere at the distance of the Earth a temperature of 27° was found. For such bodies the temperature varies inversely as the square root of the distance of the Sun. Applying this argument to the major planets, a temperature of 210° was found for Mercury, 27° for the Earth, -38° for Mars, and -219° for Neptune, so that, said Prof. Poynting, it was not conceivable that the "canals" on Mars could be filled with liquid water.

The following are some of the papers not already mentioned presented at the Meeting:—

A. ANGOT (Paris).—The Relation between the Minima and the following Maxima of Sun-spots.

Prof. K. ÅNGSTRÖM.—An Instrument for the Measurement of the Radiation from the Earth.

Z. U. AHMAD.—On the Use of Divergent Series in Astronomy.

Prof. O. BACKLUND.—Some Results of Researches on the Comet Encke.

Prof. BIRKELAND (Christiania).—On Sun-spots and Polar Auroræ.

On Plato's Theory of the Planets.*
(Republic X. 616 E.)

THERE is a passage of interest to the astronomer, and of well-known difficulty to the classical scholar, in Plato's allegory of the

* Read before Subsection A at the British Association Meeting, 1904.

Vision of Er, where the Spirits survey the heavens and the earth and all the harmonious motions of the Universe. They see the heavenly bodies set in certain revolving whorls, which, whorl within whorl, combine to form the Spinning-whorl on the Spindle of Necessity; and the Goddess holds the spindle on her knee, and spins the thread that the Fates wind, unwind, and cut.

We are told that the whorls are arranged, one within another, in the following order:—1. The Fixed Stars; 2. Saturn; 3. Jupiter; 4. Mars; 5. Mercury; 6. Venus; 7. The Sun; 8. The Moon.

This order, which is as good as any other that can be framed under a geocentric hypothesis, presents no difficulty. But we are further told, and here the difficulty begins, that the whorls differ from one another in respect of "breadth of rim":—"The first and outermost whorl is that which has its circular rim the broadest, and the sixth whorl comes next to it in regard to breadth of rim; and, proceeding in order of breadth, the fourth whorl comes third, and the eighth fourth, and the seventh fifth, and the fifth sixth, and the third seventh, and the second eighth." Thus we have now a new classification of the heavenly bodies, in the following sequence:—1. The Fixed Stars; 2. Venus; 3. Mars; 4. The Moon; 5. The Sun; 6. Mercury; 7. Jupiter; 8. Saturn.

We might expect to elucidate the passage in two ways, either by literal interpretation of the Greek, or by seeking a clue in the facts and language of Astronomy. The first hope eludes us, chiefly for the reason that Plato was not merely describing a phenomenon but was composing a riddle; and the second method has proved hitherto unsuccessful, the order given being out of accord with any scheme of classification employed by astronomers.

It is plain enough that, under the allegoric guise of spindle-whorls, Plato was representing a system of concentric spheres, such as were commonly postulated, in one way or another, by the ancient astronomers; and it appears, accordingly, to be very reasonable to seek for guidance in that system of the spheres which was elaborated by Eudoxus, who was Plato's pupil and whose theories have been handed down to us by Simplicius.

According to Eudoxus, the Fixed Stars were set all in a single sphere, which gave them their diurnal motion about an axis lying north and south. The Sun and the Moon received their diurnal motions from spheres coaxial with that of the Fixed Stars, but each required a second sphere, whose axis was fixed obliquely within the first, and whose motion was in an opposite direction, in order to carry them in their annual or monthly period through the signs. An identical mechanism accounted in part, but not completely, for the motions of the remaining planets; but, in the case of these, it was necessary to devise some further machinery such as should be capable of producing the apparent irregularities of their orbits, that is to say their "stations and retrogradations." Eudoxus, who is said to have been the first to analyse the

apparent motion of a planet into a system of interlinked figures-of-eight, and who gave to this figure-of-eight curve the name "hippopedon," accounted for this peculiar motion in a very masterly way, without departing from the general plan of a system of concentric spheres. In addition to the two spheres already indicated, he conceived each planet to be ruled by two other concentric spheres, and to be set upon the equator of the fourth or innermost. The third sphere had its poles in the equator of the second, that is to say in the plane of the ecliptic, and therefore revolved, approximately, in a north and south direction. The fourth sphere, placed within it and revolving with equal and opposite velocity, counteracted the motion of the third and prevented the planet being carried, as the third would of itself have carried it, to first one and then the other pole of the ecliptic. But the fourth sphere was not coaxial with the third, but was set somewhat obliquely to it (just as the first sphere was to the second), and thus introduced another component in the form of a simple harmonic motion, causing the planet to perform apparently a pendulum-like vibration in the plane of the ecliptic while all the while it was being carried around that circle by the proper motion of the second sphere. The phase of this vibration is obviously conditioned by the angle which the axis of the fourth sphere bears to that of the third; and Eudoxus tells us that these angles differ for the different planets, though he does not tell us what they severally were. The calculation, a very simple one, has been performed by Schiaparelli *, who shows that the angles must have been as follows:—

Venus	46°
Mars	34
Mercury	23
Jupiter	13
Saturn	6

Obvious though it be, it is worth emphasizing that these are not merely angles concealed, so to speak, in the construction of the system of spheres, but, doubled, they represent the amplitude of each vibration or the apparent visible excursion of each planet in its performance of a complete hippopedon. Now these numbers place the planets, excluding the Sun and Moon, in precisely the order of Plato's text!

Moreover, these motions of the planets are not without obvious analogy to the excursions of the Sun and Moon to either side of the equator, across the inter-tropic belt; and the analogy is all the better seen when we think of the mechanism of axial obliquities to which in each case, according to Eudoxus, the phenomenon is due. I think, therefore, that we may be justified in including

* *Le sfere omocentriche di Eudosso, di Callippo e di Aristotele*: Milano, 1875.

in our series the angle of the ecliptic for the Sun, that is to say about $23^{\circ} 28'$, or, if we assume the epoch of Eudoxus, about $23^{\circ} 45'$; and for the Moon an angle which, as Eudoxus tells us, is in her case somewhat greater than the Sun's, in fact about 5° more. Then, having intercalated these planets with their appropriate angles, we find the whole seven to follow the precise order of Plato's series. As has been suggested already, we obtain by doubling these angles the measure of that equatorial belt or zone within which the planet enjoys, apparently, a certain freedom of motion, but beyond which it can never range.

I take it, then, that Plato utilized all the astronomic science of his time, simplifying it for the purposes of his allegory. There can be no doubt whatever that Plato never supposed that a single sphere could account for the complicated motions of each planet; he merely omits from his description the invisible and ancillary spheres: he suppresses all those portions of the spheres into which the planetary orbits never range, and by this suppression is led to his metaphor of the "whorls."

As to the Fixed Stars, they extend over the whole surface of their sphere, or, in other words, the breadth of their "whorl" is a zone of 180° . So we at least should say: unless it be more reasonable, in the present case, to exclude the southern stars that were invisible to the ancients, and so to assume a zone some 30° less. In either case we reach, as our final conclusion, the following measurements of the breadth of Plato's "whorls," in Plato's order:—

1. The Fixed Stars		$150-180^{\circ}$
2. Venus		92
3. Mars		68
4. Moon		57
5. Sun		47
6. Mercury		46
7. Jupiter		26
8. Saturn		12

D'ARCY W. THOMPSON.

The Spectra of Sun-spots.*

THE paper contains a reduction of all the observations of sun-spot spectra taken at Stonyhurst during the years 1883-1901 with a 12-prism spectroscope attached to either the 8-inch or 15-inch equatorial. A discussion of the observations of the spectra of ninety sun-spots, observed during the period 1883-1889, appeared in the 'Memoirs R.A.S.' vol. l., and of twenty-four other spots

* Abstract of a paper read by the Rev. A. L. Cortie before Section A at the British Association Meeting, 1904.

observed in the period 1890-1901 in the 'Monthly Notices R.A.S.' vol. lxiii. No. 8. All the observations have been taken by the same observer, and are not restricted to a few lines, but on each occasion some particular region of the spectrum between B and D has been selected for detailed study, after a general view of this part of the spectrum had been secured for determining the most widened lines. The earlier observations of the widened lines were catalogued according to Angström's wave-length numbers, as corrected in British the Association 'Catalogue of the Oscillation-frequencies of the Solar Rays' (1878); the later observations according to Rowland's numbers. The present catalogue of 346 widened lines between wave-lengths 5884.03 and 6867.46, which combines all former lists, is based on Rowland's numbers, and contains 5486 individual observations.

The chief phenomena in the spectra of sun-spots are, as regards the general absorption, a want of uniformity of blackness in various regions of the spectrum sometimes observed, and, as regards the selective line absorption, the widening of lines, darkening of lines without widening, displacement of lines, obliteration of lines, extension of the widening through the penumbra, reversal of lines, hazy fringes to lines, and spot-bands. The tables accompanying the paper contain, the one, the mean relative widening of the lines of the chief elements identified, and the other, a list of the most widened lines.

The tables show the importance of faint lines of Vanadium and Titanium in the sun-spot spectra ('Monthly Notices R.A.S.' vol. lviii. No. 7). These faint lines have been always, and at all times of the sun-spot period, among the most widened lines (*loc. cit.* vol. xlix. No. 8; vol. lxii. No. 7). The observations give no evidence of a crossing of the most widened lines at an epoch between sun-spot maximum and minimum. They show, however, that the iron lines are more affected in minimum than in maximum spots; but no conclusion can be drawn as to a difference of character and temperature between maximum and minimum spots from the behaviour of such faint widened lines. The iron lines brightened in the chromosphere, which are mostly arc lines, are not differently affected in sun-spots from lines not brightened. The widening of some oxygen lines in sun-spots in the α band seems to be a real phenomenon, the single hydrogen line (C) is generally thinned, and almost reversed over spots, and frequently reversed and distorted in their immediate neighbourhood. If oxygen and hydrogen are present in sun-spots, water-vapour might be formed over them. Spot-bands sometimes seen (*loc. cit.* vol. xlvii. No. 1) are a probable witness to a reduction of temperature sufficient for the formation of compounds. But the widened lines accredited to water-vapour occur generally in crowded parts of the spectrum, so that the widening may be really due to faint solar lines in juxtaposition with them. The predominance of Vanadium and Titanium in sun-spots is important in view of Mr. Fowler's recent identi-

fication of the flutings in Secchi's third-type stars as due to titanium or a titanium compound, and Sir Norman Lockyer's matching of the lines intensified in the spectrum of Arcturus with lines of the same element. Professor Hale has also shown that many of the lines in the fourth-type stars are coincident with lines observed as widened in sun-spots by Mr. Maunder and myself.

Recurrent Markings on Jupiter.

IN looking over and comparing, as far as could conveniently be done, several thousand drawings of the planet Jupiter made during the last half-century, I could not help being struck with the fact of similiarity in the markings (and their latitudes) presented, in many instances, at widely separated intervals. From the large amount of evidence to hand it appears certain that features exhibiting various peculiarities of appearance and rates of motion are common to certain latitudes and break out from time to time, enduring for certain unknown intervals, then disappearing to be replaced by similar phenomena. There are no doubt some exceptional outbreaks which only take place at long intervals. There are others the evidences of which remain visible for lengthy periods. The slant belt on Jupiter in the spring of 1860 and the rapidly moving dark spots in N. temp. region in the autumn of 1880 offered examples of occasional phenomena. The red spot appears to supply us with a feature of very extended duration and one which exhibits its periods of appearances and disappearances. If old drawings could be collected and a large number compared, the result might be interesting, as affording us a closer insight into the wonderful atmospheric phenomena of Jupiter.

There must be a vast number of drawings of the planet available for discussion. Schwabe made large numbers of delineations between 1830 and 1860, and Schmidt obtained 300 or 400 more between 1843 and 1880. If these and many others could be brought together and suitably discussed they would throw considerable light on the physical changes taking place.

The importance and value of the observations must, however, in a measure depend upon their continuity, and whether the individual features are capable of being traced from opposition to opposition: irregular breaks of several years in the observations would destroy their value. For this reason it might be almost advisable to wait until further observations have been made. In recent years the planet has been carefully followed, the principal features drawn and their rotation periods determined by a number of reliable observers. When these researches have been further extended, we shall learn much from a comparison of the whole number in their regular sequence. But there is no doubt that, without waiting for another generation to pass, much might be learned from the old records by Schwabe, Schmidt, and other observers who worked many years ago.

Bristol, 1904, Sept. 4.

W. F. DENNING.

CORRESPONDENCE.

*To the Editors of 'The Observatory.'**Thomas Streete.*

GENTLEMEN,—

So little is known about the author of '*Astronomia Carolina*,' that his name does not occur in the '*Dictionary of National Biography*.' Sir George Airy did not often indulge, like our late friend Captain Noble, in pleasantries at scientific meetings; yet I do well remember his once saying at the R. A. S., speaking of Streete and an instrument which had formerly belonged to him, that "one would almost imagine from his name that he had been an ancestor of our present worthy secretary," meaning De la Rue, who was present.

Streete observed the transit of Mercury which occurred on the 3rd of May (23rd of April O.S.), 1661; it was also witnessed by Hevelius at Danzig, and by Huygens, who happened then to be in London. It is well known that the first to observe the whole of one of these phenomena (i. e. the ingress and the egress) was Halley, in November 1677, and that his observation of it at St. Helena led him to suggest those of transits of Venus as furnishing the best means of determining the distance of the Sun. According to Wing*, Streete, Mercator, and Huygens observed that of 1661 at Long Acre; and Huygens made a measurement of the planet's distance from the Sun's limb.

The '*Astronomia Carolina*' appeared in 1661; the author states, in the dedication to the king (Charles II.), that it had been begun some years before the restoration, during "the sad and doleful Night of Your Sacred Majestie's absence from your People," but was completed soon afterwards; and he is careful to note that the transit of Mercury occurred on the day of Charles's coronation. The dedication is followed by an address to the reader, "whose grateful acceptance may (in convenient time) produce a larger return of endeavours" from the author. His tables, however, did not meet the approbation of Vincent Wing, whose '*Astronomia Britannica*' had appeared in 1652, and his '*Astronomia Instaurata*' four years afterwards. Commencing with 1652, Wing published a series of ephemerides; in that for 1663 he gave a discourse about parallax and refraction, respecting which Streete cautioned him that some of his suppositions were "groundless and wide of truth"; but as he repeated and defended his statements in the almanack for 1664, Streete, in an Appendix to the '*Astronomia Carolina*,' put forth a "Monitum to Mr. Vincent Wing," in which he points out, truly enough, that refraction produces the same amount of effect upon Venus and the Sun when both are near the horizon. Wing replied in a tractate entitled '*Examen Astronomiæ Carolinæ*,'

* '*Astronomia Britannica*,' p. 312.

the motto of which is "Prius intellige, et tunc increpa." Streete retorted in 1667 with 'Examen examinatum,' the motto of which is "Humanum est errare, perseverare diabolicum," giving what he calls "a castigation of the envy and ignorance of Vincent Wing." He points out, amongst other things, that Horrox had determined the horizontal parallax of the Sun to be 14", or at the most 15", and Huygens only 11", yet Wing took it to be 2' 21" or 141", much more than double what Kepler or Hevelius had considered it. Streete's last publication was 'The Description and Use of the Planetary Systeme,' which appeared in 1674. At the end of this little treatise he defends himself from some remarks by Flamsteed in a letter to Mr. Collins which was published in No. 110 of the 'Philosophical Transactions.' Wing, who, according to Rigaud, had had some correspondence with Flamsteed, had died in 1668. An account of him is given in the 'Dictionary of National Biography', but I have not been able to ascertain anything further about Streete. In the year 1705 appeared, at Nuremberg, a Latin translation of his work by Prof. J. G. Doppelmayr, who gives no further information respecting the author, but says that he translated the 'Astronomia' "ob insignem utilitatem ex Idioma Anglicano in latinam Linguam," and adds as an Appendix a copy of the Rudolphine Tables. "Interim," he says, however, in his preface, "Tabulæ Streetianæ laude sua non defraudandæ, cum hæ Nicolao Mercatore aliisque testibus, in motu præcipuè Mercurii imprimis sint commendandæ, et, si Bullialdini in hoc passu non præferendæ, tamen æquiparandæ." With this high praise of our author I conclude, and remain

Yours faithfully,

Blackheath, 1904, Aug. 6.

W. T. LYNN.

P.S.—In 1716 (Weidler says 1710, but that is an error) Halley published a new (third) edition of the 'Astronomia Carolina'; in the preface he speaks in high terms of the work; and alludes to the author as dead, but does not mention the date of his death.

· Meteoric Observations.

GENTLEMEN,—

Meteoric observations appear to me to be particularly required in the first half of the year, but meteors are very scarce during this period, and the more feeble showers are only to be discovered with extreme difficulty and by watching for long intervals. Zezioli and Tupman made many observations in the winter and spring months, but certain other observers appear to have rather neglected these periods. It is true that watching is often rather tedious; four or five meteors in an hour are scarcely sufficient to maintain the ardour of the observer and prompt him to keep long vigils at a time when he is shivering in the bleak,

* I may add that, in the historical matter at the end of 'Astronomia Britannica,' he tells us that the solar eclipse (which was nearly total) on the 11th of May (O.S.), 1621, was observed by his own father (also Vincent Wing).

frosty mornings. Apart from the discomforts and the rarity of meteors, however, there remains a lot of good work to be performed in the meteoric department at the seasons mentioned.

It is hoped that some observer sufficiently enthusiastic and able will specially devote himself to the study of the various showers displayed in the skies from January to June. We have already pretty thoroughly learnt the positions of radiants active at the period of the Lyrids, and a list of all the chief centres was published in the *Observatory* for August 1904.

Meteoric apparitions, if not so frequent in the early part of the year, are every bit as important and interesting as those witnessed in the last half, and they certainly require further careful investigation. A great number of observations have been effected in July, August, and succeeding months, when meteors are tolerably numerous, but the winter or spring seasons rarely find the meteoric observer at his post. In the third week of April he stands out to greet the coming of the Lyrids, but after their brief display is over he relapses into inactivity until the long-pathed Aquarids and swift, streaking Perseids begin to show in the sky during the third week of July. In fact, several of the observers appear to exert themselves only at the periods of specially strong showers. What we require, however, is much increased observation at other periods, for we already possess an enormous number of records for the periods of the Quadrantids, Lyrids, Perseids, Orionids, Leonids, and Geminids.

It is, of course, desirable that these important annual streams should be watched from year to year, but it is at other periods that we really require further materials for deducing the radiants. It is hoped, therefore, that meteoric observers will consider this point and direct their efforts accordingly. The best plan for an habitual observer to pursue is to watch on clear, moonless nights, irrespective of the date, and to ascertain the strength and radiant-positions of such showers as are presented. The method of selection previously adopted in regard to the richer showers has had the effect of making certain periods superabundant in materials, while other periods have very few observational records from which their meteoric phenomena can be deduced.

Yours faithfully,

W. F. DENNING.

Bristol, 1904, Sept. 3.

The First Director of the Sydney Observatory.

GENTLEMEN,—

The approaching retirement of Mr. Russell, who has so long and so ably directed the observatory at Sydney, leads me to offer you a few lines about his first predecessor.

William Scott was born at Hartland, North Devon, on the 8th of October, 1825; he took his degree at Cambridge as third Wrangler in 1848, and, being elected a Fellow of Sidney Sussex

College, was afterwards ordained. In 1853 he published an 'Elementary Treatise on Plane Co-ordinate Geometry,' in the treatment of which acknowledgment is made to the (now late) Prof. Salmon's work on Conic Sections. When it was determined by Governor Sir William Denison to establish an observatory in Sydney, the post of Director was offered, on the recommendation of the Astronomer Royal, to Mr. Scott, who accepted it.

The first founder of an observatory in Australia was Sir Thomas Brisbane, who, when Governor of New South Wales in 1822, constructed and furnished one at Paramatta, a few miles to the west of Sydney. He was assisted by C. Rümker and Dunlop, and much good work was accomplished there, which led to the award of the Royal Astronomical Society's Gold Medal to Sir Thomas in 1828. In particular, it will be remembered that at Paramatta alone was observed Encke's Comet at its first predicted return, Rümker obtaining a series of observations from June 2 to June 23, 1822, the results of which were published in vol. ii, col. 7 of the *Astronomische Nachrichten*. The observatory career continued until 1847, when it was dismantled, but not much was done there during the later years of its existence. For more than ten years Australia remained without an observatory.

The Rev. W. Scott arrived at Sydney on the 31st of October, 1856, and at once proceeded to select a site for the new observatory. It is on the south-west side of the town, in a government reserve now called Observatory Park, and about 146 feet above the level of mean water. The building was commenced in May 1857, and sufficiently advanced to admit of making meridian observations (with an old instrument removed from Paramatta) for determination of time in June 1858. In the following December Mr. Scott presented to the Observatory Board his first Annual Report. About the same time a new transit-circle arrived from England, and regular observations were commenced with it in June 1859. In the following year, 1860 (the same in which Sir Thomas Brisbane died in January), appeared a first volume of astronomical observations, the principal objects being the Moon and stars on the meridian. The establishment being afterwards provided with an equatorial, Mr. Scott obtained observations of Comet III. 1860 (which suddenly became visible in the southern hemisphere about the end of June), of the magnificent Comet II. 1861 (which was discovered by Mr. Tebbutt on May 13, and in the tail of which the Earth was involved about the end of June), of the transit of Mercury in November 1861, and of Encke's Comet in February 1862. Later in that year he resigned, and was, after an interval, succeeded by Mr. G. R. Smalley, formerly Lecturer in King's College, London, who had before that had some experience in observing at the Cape. Mr. Smalley, however, died not long after he had taken charge, and this led to the appointment of Mr. H. C. Russell, the recent announcement of whose retirement we all regret; but a time comes to each in turn to be "rude donatus," like Horace in another way.

After Mr. Scott had resigned the directorship of the observatory, he returned for a while to England, but soon went again to Sydney, where he became Warden of St. Paul's College at the University in 1865, holding that post for eight years. He was also Secretary of the Royal Society of Sydney from 1867 to 1874, and its Treasurer from 1875 to 1878. He was nominated to a canonry in St. Saviour's Cathedral, Goulburn, in 1880, and, after being vicar in succession of Gunning, Queanbeyan, and Bungendore (all in New South Wales), died at Sydney in the year 1889.

Yours faithfully,

Blackheath, 1904, Sept. 19.

W. T. LYN.

Jupiter apparently Moonless.

GENTLEMEN,—

The Rev. S. J. Johnson, in his interesting letter published at pp. 313-4 of your August number, refers to an instance of the above phenomenon which occurred on the 21st March, 1874, between 19^h 4^m and 20^h 57^m G.M.T., and was remarkable for the prolonged disappearance of the satellites. He adds that he does not remember any account of this case having been observed in the Southern Hemisphere.

It may therefore be of interest to state that it was observed in the Western Hemisphere, at Amherst, Mass., by Prof. Todd, who saw Jupiter, on 21st March, apparently moonless for nearly two hours, from about 14^h to 16^h local astronomical time (see his book 'Stars and Telescopes,' p. 123). I published a brief note on this subject in the 'Journal of the British Astronomical Association,' vol. x. p. 223, March 26, 1900.

This 1874 example is complementary to that of 1867. In 1874 II. was in front of, and I., III., IV. were behind the disc; but in 1867, II. was behind, and I., III., IV. were in front of the disc.

Invermay, Hyde Park, Leeds.

1904, Sept. 23.

Yours faithfully,

CHAS. T. WHITMELL.

OBSERVATORIES.

THE following notes on work at various Continental Observatories are extracted from the 'Vierteljahrsschrift der Astronomischen Gesellschaft,' and refer to the work during the year 1903.

BAMBERG. *Ernst Hartwig.*—On account of the unfavourable weather, the parallax measures with the heliometer had to be given up. The 10-inch refractor and the 6-inch Merz were used for variable stars on 98 nights. During the year there were 700 visitors.

BERLIN. *W. Foerster.*—The transit-circle was, as in former years, entrusted to Herr Battermann. Occultations were observed with the 6-inch refractor, while the heliometer was employed in measuring the solar diameter. Time-service.

BERLIN (Astronomical Computations). *T. Bauschinger*.—The 'Jahrbuch' for 1906 has been printed. Table of elements for small planets 1 to 512. Accurate ephemerides for the opposition of 41 planets and approximate ephemerides for another 68. A mass of other computations in relation to minor planets.

BONN. *F. Küstner*.—A series of photographs of Nova Persei with the object of determining the parallax. Observations for the revision of the Bonn Catalogue have been continued.

BRESLAU. *J. Franz*.—Observations, with the 6-inch refractor, of comets 1902 *d* and 1603 *c*. A few double stars were measured by Dr. Lachmann. The $3\frac{1}{2}$ -inch transit was used for clock-stars and Moon culminators. The 3-inch heliometer was employed in measuring points on the lunar surface. Meteorology.

DÜSSELDORF. *W. Luther*.—The ring-micrometer on the Merz refractor (186 mm.) was used for small planets on 55 nights, 89 observations of 35 planets being made. Variable stars.

GENEVA. *R. Gautier*.—Time-service. The number of pocket chronometers and watches tested in 1903 was 266, being 93 less than in the previous year. The Plantamour equatorial, as in former years, was employed in observing comets and small planets. Comets observed being 1903 *a*, 1902 *d*, 1903 *c*. The observations are in the *Astronomische Nachrichten*. Observations of Nova Geminorum. Meteorology.

GÖTTINGEN. *K. Schwarzschild*.—The heliometer was used for work on the Orion Nebula and a few double stars by Dr. Meyermann, and by Dr. Ambronn for measures of the solar diameter, diameters of Jupiter, Saturn, Venus, and Mercury. Photometry of stars in Præsepe and Coma Berenices. Solar spectroscopy.

HAMBURG. *R. Schorr*.—Re-reduction of Rumker's catalogue. 530 light estimations of 22 variables, including Nova Persei and Nova Geminorum. Sixty-two observations of comets 1902 *d*, 1903 *a*, 1903 *c*. A large number of observations of small planets. Time-service. Meteorology. Publication of the *Astronomische Nachrichten*.

PUBLICATIONS.

AN INTRODUCTION TO THE STUDY OF SPECTRUM ANALYSIS*.—Prof. Marshall Watts some years ago conferred a benefit upon spectroscopists and others by his handy 'Index of Spectra.' The present book contains a catalogue of spectra brought up to date,

* 'An Introduction to the Study of Spectrum Analysis.' By W. Marshall Watts, D.Sc. (Lond.), F.I.C. Longmans, Green, & Co., 39 Paternoster Row, London, New York, and Bombay, 1904.

but in addition half the book is devoted to a history of spectrum analysis, and to a description of the chief departments of its application to astronomy. The entire book is not a large one, and thus the amount of space devoted to so wide a subject seems very small. Nevertheless, within the narrow limits which Prof. Watts has assigned to himself, he has accomplished his task with remarkable success. There are a few signs at the beginning of the book which seem to indicate that, when he commenced it, the author intended it to be of quite an elementary character throughout. He writes on page 4 as if it were too much to assume that any of his readers had ever opened an elementary book on *Plane Trigonometry*. This was, however, too low a note to be maintained throughout, and there is a certain unevenness of standard noticeable in consequence. But this scarcely detracts from the excellence of the book as a whole. The earlier chapters describe the prismatic spectroscope, flame-spectra, and spectra produced by means of electricity, the diffraction spectrum, and the determination of wave-lengths. Then follow chapters on the applications of the spectroscope to astronomy, in which the leading results are very ably summarized. The five chapters which follow are of especial value, inasmuch as for one reason or other they deal with subjects found only imperfectly or not at all in other text-books. The concave grating, the arrangement of the lines of the spectra of many elements in a series bearing definite arithmetical relationships to each other, the true nature of the spectrum of the Bunsen flame, known as the "Swal:" spectrum from one of its earliest observers, the Zeeman effects, and the theory of échelon gratings are subjects of this class, and are here dealt with in a clear manner and with a fair degree of fulness.

The book is very thoroughly illustrated, having a coloured plate of the solar spectrum as frontispiece, and 135 illustrations in the text. The latter are for the most part useful, but lend themselves to criticism in some cases. There is but little reason for reproducing in so small a book, and one devoted to so wide a range of subject, numbers of photographs of nebulae, prominences, comets, and coronæ which, however beautiful, are tolerably familiar to the student, and are only germane to the subject in a secondary degree. The reproductions of photographs of spectra are usually dull and blurred, and by no means show the details well. Some of the smaller woodcuts, too, are very old and familiar, and could easily have been replaced by less hackneyed designs. These are, however, questions rather of taste than of importance, and there can be no doubt that Prof. Watts has prepared a most useful and needed text-book in a very compact and convenient form.

THE MATHEMATICAL THEORY OF ECLIPSES *.—It is to be feared that the number of readers who care to thoroughly investigate all

* 'The Mathematical Theory of Eclipses.' By Roberdeau Buchanan. J. B. Lippincott, 1904.

the details of a solar eclipse is not very large. The subject may, however, be recommended to geometers from the neat relations involved, especially in the Besselian and kindred methods. The writer of this book is very well qualified for his task, as he has computed all the eclipses in the American Ephemeris for 23 years, and can warn others of many traps into which they are likely to fall. The work is largely based on Chauvenet, but the order of the sections is altered to accord better with practical work, and there are many interesting digressions on numerical values and other points. There is a re-discussion of eclipse limits, a matter on which authorities differ much; also of the changes in the adopted semidiameter of the Moon, and of the recently-noticed fact that total solar eclipses require a smaller semidiameter than occultations, in consequence of the irregularities in the Moon's limb.

All the ordinary problems, such as finding north and south limits of total or partial eclipse, curves where eclipse begins or ends with the Sun in the horizon, &c., are fully explained and illustrated by examples. A number of singularities in eclipse-tracks are noted, such as eclipse of the midnight Sun, north limiting curve of totality south of south limiting curve, the north limit of an eclipse, being also the nearest point to the south pole of the limiting line, &c. The last two points may serve as astronomical puzzles; the solutions will be found in the book.

Graphical methods and the construction of eclipse-charts are also explained, and the advantages of various projections are discussed.

There is some mention of the *Shadow Bands*; we are sorry to see the untenable diffraction-theory of their origin mentioned though not endorsed; it is decisively negatived by their moderate velocity. The theory that they are due to rapidly-changing temperature is ascribed to Prof. Bigelow's work on the 1900 eclipse, but he was not the first to make the suggestion. It is at least as old as March 1896 (*vide* 'Journal of British Astronomical Association,' vol. vi. p. 282).

Mr. Buchanan raises as an objection to the temperature explanation, that the temperature falls through a large portion of the partial-eclipse region, but the bands are not seen here; but evidently their visibility requires the Sun to be reduced to a mere line of light, in addition to the wave-movements in the air produced by changing temperature.

We are glad to see that the effect of refraction on eclipses is discussed, as this point is often overlooked. For example, in the method of reducing occultations used at Greenwich, no account is taken of refraction, though with a low Moon its effects may be much greater than many of the small corrections that are applied.

Final chapters give briefly the methods of calculating transits of Mercury and Venus, and occultations of stars by the Moon.

Under the latter heading the author points out how little information the "limiting parallels" give as to visibility at a particular station, and suggests that several points should be given on the north and south limits for each occultation. This would be very convenient to observers of occultations, but would involve more computations and more printing, so that it can scarcely be hoped that the almanacs will adopt it.

We may point out a few misprints and errata. On p. 32 is the statement "only 3 eclipses can occur at each node, 2 solar and 1 lunar, *or the reverse*"; the words in italics imply the possibility of two lunar eclipses at a node, but this is impossible unless we include penumbral lunar eclipses, which is contrary to ordinary usage.

On p. 35 *d* is printed for *h*, and 11^h and 12^h should be interchanged.

The note on p. 55 (itself a correction of a mistake) has three proper names mis-spelt—Schillen, Lasselle, Huggens.

The statement at the top of p. 61 *re* the motion of eclipse-tracks in the Saros, "in time the path, instead of moving south across the Earth, will move north," is very misleading. There may indeed be a temporary reversal of direction of motion, but the above passage suggests a permanent reversal which cannot take place.

There is another mis-statement on p. 61 *re* the series of total eclipses 1860, 1878, 1896: it is stated that the last total eclipse of the series will be in 1932, whereas it will really be in 1986.

A. C. D. C.

KATALOG DER ASTRONOMISCHEN GESELLSCHAFT (Zweite Abteilung, Zweites Stück: Zone -6° bis -10° . Wien: Ottakring).—The publication of this catalogue forms an extension of the chain of catalogues organized and published by the Astronomische Gesellschaft at Leipsic, and forms one of a second series; the first series was arranged to contain 15 catalogues from Dec. $+80^\circ$ to -2° , all of which, except the second section, $+74^\circ 40'$ to $+70^\circ 10'$, are published.

The observations of these 8468 stars appear to have been made expeditiously; the first zone observed was in 1892, Jan. 19, and the last, omitting revision work, in 1899, Jan. 12; but the bulk of the work was done in 1893-5. The instrument used was the Kuffner Observatory meridian-circle which was moved to Ottakring; the aperture and focal length are just under 5 inches and 5 feet respectively, and the magnifying-power was 120. With this instrument a 9.5-mag. star would not be an easy observation; though there are few stars so faint as this, we notice that nearly 30 per cent. are fainter than 9.0, except in the Milky Way, near 6^h , where the percentage falls to about 15 per cent.

The places of the reference stars have been computed from Auwers' Fundamental Catalogue for Southern Zones and are mostly brighter than the 6th magnitude; about a page is given to the discussion of the magnitude equation.

We notice that between 80 and 90 per cent. of the stars in the catalogue have only two observations; it would have been more in accordance with modern requirements if a third observation had been made in all cases, the probable error of a position would have been much decreased, and this would not have seriously extended the time required for observation and reduction, or the expense. The mean error of a single observation, also the probable error, is stated to be as follows:—

	Stars ≥ 8.5 mag.		Stars ≤ 9.1 mag.	
	R.A.	Dec.	R.A.	Dec.
Mean error of a single obs.	$\pm 0''.038$	$\pm 0''.65$	$\pm 0''.050$	$\pm 0''.74$
Probable error of a single obs. ...	$\pm 0''.026$	$\pm 0''.44$	$\pm 0''.034$	$\pm 0''.50$

from 960 stars.

The most important point that attention should be directed to is the adoption of the epoch 1900.0. In view of the same epoch having been adopted by all observatories co-operating in the photographic survey, this is of immense importance, and it is to be hoped that all star catalogues for the next 10 years or more will be referred to this epoch. Looking back on past years one regrets, although the inception of the Astrographic Catalogue could scarcely have been foreseen, that this epoch was not chosen from the beginning of this A. G. catalogue scheme, as much time, labour, and expense would have been saved to those using these catalogues for photographic plate reductions.

No list of zones or comparisons with other catalogues are given, as in similar catalogues; the Third Radcliffe Catalogue covers the whole of this zone, and it is hoped to make a comparison with that work for communication to the Royal Astronomical Society in November.

We close this brief review with praise for the excellent printing in this catalogue, which may be extended to all the rest of the series already published.

F. A. B.

NOTES.

COMET NOTES.—An object that was supposed to be Encke's Comet was photographed at Heidelberg on September 11 with the Bruce telescope, the exposure being $3\frac{1}{2}$ hours, and the object being noted as very faint and diffused. The discovery was subsequently noted as somewhat doubtful, and apparently no further observations have been obtained. An ephemeris was given in the August number.

A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered :—

Planet.	Date.	Discoverer.	Place.	Mag.
OP	1904 Aug. 14	Wolf.	Heidelberg.	13.5
OQ	15	Götz, Kopff.	"	11.8
OR	Sept. 6	Kopff.	"	13.3
OS	5	"	"	12.2
OT	11	Götz.	"	12.1
OU	} Details not yet to hand.			
OV				
OW	1904 Sept. 19	Götz.	Heidelberg.	11.0

In *Ast. Nach.* 3966 Hans Osten shows that the orbits of planets 505, 521 approach one another within $\frac{1}{3}$ of a million miles, and that the two planets made a close approach in November 1903.

A. C. D. C.

SATURN'S NINTH SATELLITE.—It is announced in *Ast. Nach.* 3969 that this object, Phœbe, has been seen by Prof. Barnard with the 40-inch telescope of the Yerkes Observatory. We have been informed that Prof. Turner also saw the satellite when at Yerkes, evidently since the note on p. 390 was written.

From the rather scanty information respecting the satellite yet to hand, viz., the observations of 1898 August, the places of July last published by Prof. Pickering, and Prof. Barnard's observation, Mr. Crommelin has kindly deduced for us the following very approximate elements. Plotting the observed places, and assuming that the satellite is moving "direct" and in a circular orbit, the plane of the orbit is inclined about 6° to the ecliptic, which is in accordance with the 'Harvard Circular' of 1899. It passed inferior conjunction about August 27, and its linear motion relatively to Saturn was then one-third that of Titan, so that, assuming that the orbit is circular, its distance from the planet is nine times that of Titan. In six years and a month it has moved through a certain whole number of revolutions $+162^\circ$, allowing for Saturn's motion in the interval, and if the whole number be taken as 5, a sidereal period of 410 days follows. The hypothesis of Retrograde Motion round Saturn was next tried, and it was found that this represents the position-angle of 1898 much better, so that it is conjectured this may be the peculiarity in the orbit referred to in the Oxford Note-Book of last month. On the assumption of five complete revolutions since 1898 August, the sidereal period becomes 443 days and the distance 6,960,000 miles. Inclination to ecliptic about 6° , to Saturn's orbit $4\frac{1}{2}^\circ$, to Saturn's equator 30° .

Prof. Barnard gives its magnitude as 16.7. From the photographs of 1898 it was said to be 15.5, and assuming its reflecting power to be the same as that of Titan, a diameter of 200 miles was deduced. The new observation would make its diameter about 120 miles.

It is certainly the faintest object of the universe which has been under observation, if we except nebulae. The magnitude of Jupiter's fifth satellite is given as 13, so that the linear dimensions of the two bodies are approximately the same.

THE NATURE OF THE SOLAR CORONA.—Bulletin No. 58 of the Lick Observatory is signed by Prof. Svante Arrhenius (it is not stated what connection he has with the institution), and deals with some measurements of the heat and radiation of the solar corona made by Langley and Abbot. His conclusion seems to be that the corona consists of particles of metallic dust, possibly molten iron, driven off from the Sun by radiations, and are of such a size that they remain swimming around the Sun under the action of gravitation and the pressure of radiation. Of other particles of similar matter, not of this particular size, the larger are drawn back into the Sun, the smaller are driven off by the pressure of radiation into space. We believe that this is not the first publication of Prof. Arrhenius' theory, but it is here submitted to numerical treatment.

DRY SUMMERS.—The rainfall during the three summer months June, July, and August was this year in striking contrast to that of the same months in 1903. The total fall recorded at Greenwich in the three months of 1904 was 4.34 inches, whereas in the preceding year it exceeded 16 inches. The average rainfall for the three months, taken for the 64 years for which records exist, is 6.765 inches; this is the twelfth time in which the total for the three months was less than $4\frac{1}{2}$ inches. In several of these years it was less than 4 inches, and in three of these it was less than 3 inches. The summer of 1864 was the driest on record, with a total rainfall of $2\frac{1}{2}$ inches. The rainfall at Greenwich for September just past amounted to 1.33 inches, which is about an inch below the average.

THE ROYAL ASTRONOMICAL SOCIETY OF CANADA.—This is the Toronto Astronomical Society, whose publications have often been mentioned in these pages, under another name. It appears from the book now before us that in February 1903 His Majesty the King was graciously pleased to grant permission that the title at the head of this note might be adopted. This first Report under the new name covers two years, 1902 and 1903, and as the papers are so many it has been found necessary to abbreviate. For the same reason we can scarcely give all the names here, but a valuable catalogue of aerolites by Mr. Monck may be mentioned, which gives a large number of authenticated falls of these bodies from the year 1212 to the present time, tabulated in order of day of the year, and also another paper following this, on a similar subject, by Mr. Arthur Harvey. There is a memoir entitled "Woman's Work in Astronomy," which is interesting, but which contains a curious state-

ment which it may be well to correct. The lady author says:—*"At the Paris Observatory Madame Flammarion is said to act as her husband's secretary...."* It may be well to point out that M. Flammarion does not hold an official position as a French astronomer.

THE LEEDS ASTRONOMICAL SOCIETY.—This Society suffered two severe losses during 1903 by the deaths of Mr. Teasdale, the sometime Treasurer, and Mr. Barbour, the Honorary Secretary, who had been a member of the old Leeds Society, founded in 1859 under the honorary presidency of Sir G. B. Airy. The Journal of Transactions for the year shows no falling off in quality. The excellent quality of the book may be judged from the fact that Mr. C. T. Whitmell contributes several articles, Dr. H. C. Pocklington explains the method of making glass specula, and Mr. Dodgson has an interesting paper, with illustrative plate, about astronomical symbols on coins of the ancients. The Society numbers only 91 members, but there are many good names in the list. The work of this Society is certainly of a high order.

DR. COMMON'S 5-FOOT REFLECTOR.—This instrument, which has been mentioned in our advertisement pages for some months past, has found a new home. From information given by Prof. E. C. Pickering in 'Harvard Circular' No. 83, it seems that the question of buying this telescope for Harvard was considered, but the price was prohibitive. An anonymous donor, however, gave twenty thousand dollars to the observatory, without any conditions as to its use, so that negotiations were again renewed, this time through Prof. Turner, who, as our readers know, is in America. Mr. T. A. Common, son of the late Dr. Common, on his side was so generous that Prof. Pickering says he must be regarded as having contributed a large portion of the cost; and the instrument therefore goes to America, where it will be used to complete the Harvard photometric survey of the heavens, so as to include the faintest possible stars.

BEN NEVIS OBSERVATORIES.—It is announced that the high-level observatory is to be closed. The Treasury offered to contribute £350 annually to their support, but this is not sufficient to maintain both observatories. The annual cost of the high- and low-level observatories together is about £1000, three-fourths of which is spent on the upper building.

TIME IN INDIA.—It is proposed to adopt as Standard Time for use upon all Indian railways and telegraphs the time of the meridian $5\frac{1}{2}$ hours east of Greenwich, and for Burma that of the meridian $6\frac{1}{2}$ hours east. But would it have been impossible to arrange a scheme in accordance with the Universal Time System,

in which the meridians exactly 5 and 6 hours east would have been used?

ON the occasion of the Meeting of the British Association at Cambridge, the University conferred seventeen honorary degrees. The list of recipients contained several names connected with astronomy—Prof. Backlund, Sir David Gill, Sir Norman Lockyer, Major MacMahon, and Prof. Schuster. Subjoined are the speeches made by the Public Orator in presenting some of these:—

Prof. O. BACKLUND.—Ab exteris exorsi, primum omnium salutamus nuntium quendam sidereum, ab arce illa celeberrima prope Petropolin stellis observandis dedicata ad nos devectum, quæ trium deinceps Struviorum nominibus iam dudum gloriatur. Ipse talium virorum hæres dignissimus, planetarum potissimum in moles motusque perturbatos diligenter inquisivit, et Enckii præsertim cometen, ter in quoque decennio inter sidera nostra lucentem, indagandum sibi sumpsit. Dum cometæ illius reditum in mensæ proximo spe certa expectamus, sideris illius indagatorem indefessum hodierni diei inter lumina libenter numeramus.

Sir DAVID GILL.—Ad patriam reversi, quam libenter salutamus virum in stellis observandis insignem, qui inter Aberdonienses suos astronomiæ studia olim auspicatus, planetæ Veneris transitum in oceano Indico accurate observandum curavit. Idem et Aegypti et Africae Australis coloniæ extremæ et Terræ Natalis spatia ampla dimensus est; stellarum omnium imaginum lucis ipsius auxilio reddendarum auctor fuit assiduus; Africae denique in promontorio remotissimo arcem cælestem sibi creditam quinque et viginti per annos fortiter et feliciter occupavit. In excubiis illis patria procul tolerandis quam fortem ipsum, in alios quam generosum sese præstitit; aliorum labores quantis stimulis incitavit, ad exitum felicem perductos quanta benevolentia excepit! Viri talis sub auspiciis et unius e professoribus nostris sub præsidio pro scientiarum societate Britannica in annum proximum Bonæ Spei in Promontorio bene nominato licet omnia fausta augurari.

Sir NORMAN LOCKYER.—Inter astronomiæ et scientiæ physiciæ fines patet provincia, ubi, instrumentis subtilissimis adhibitis, etiam solis ipsius radii retextuntur, et, linearum varietate quadam minutissima observata, corpora prima, e quibus sol ipse est compositus, inter sese distinguuntur. Adest vir in regione tam pulchra exploranda inter principes numeratus, qui, ne his quidem finibus contentus, non solis tantum defectus identidem observavit, sed etiam astronomiæ provinciæ amplissimam sibi vindicavit. Idem, per annos prope quinque et triginta Actis quibusdam præclaris luculenter editis, anni cuiusque septimo quoque die rerum naturæ totius varietatem orbi terrarum patefecit.

By inspection of the pages which form the cover of this number it will be seen that Mr. Common's advertisement is replaced by one relating to the instruments of the late Dr. Roberts. Perhaps some wealthy person may feel inclined to follow the American example. We have had an opportunity of seeing the catalogue of instruments for sale at Crowborough, and there are many small instruments and apparatus which would be disposed of separately, and might be useful to someone who sees this.

WE have to welcome a new contemporary—*Discovery*, a Monthly Magazine of Popular Science, Literature, and Art. The first issue, the September number, contains two excellent astronomical articles,

one on astronomical spectroscopy, by Mr. A. A. Buss, the other by Mr. A. C. D. Crommelin on the Solar Eclipse of 1905, August 30. The price per number is threepence.

DR. HANS BATTERMAN, of the Berlin Observatory, has been appointed Director of the Observatory of Königsberg, and Professor of Astronomy in the University.

PROF. TURNER sails from Boston for England on October 5.

THE next Meeting of the British Astronomical Association will be on Wednesday, October 26. It is probable that Mr. A. C. D. Crommelin will be the President for the coming Session.

FROM AN OXFORD NOTE-BOOK.

By the way, I saw in the papers two or three days ago that a lady of spiritualistic tendencies claims to have visited Neptune in a trance:—

She traveled faster than sight, sound, or the Sun's rays, and what she saw on her trip makes Jules Verne's vivid imagination look like the proverbial 30 cents. . . . The people there are 3 times as large as mundane beings, but they are 3 times as beautiful. The women are simply beyond description. Nobody works on Neptune, and the residents live a life of ease. Brotherly and sisterly love absolutely dictate. . . . She was particularly impressed with the sunsets. The whole surface of the distant planet was iridescent when old Sol went down: and this is evidence that they have day and night up there. . . . On her way back to Earth, Mrs. Davis saw many worlds, which are not mentioned in books on astronomy. She passed by the Moon at a swift clip, but she was able to see that there was not only one man there, but that it is generally inhabited.

A COUPLE of years ago many of us would have been puzzled, if asked what was the main work of the Albany Observatory at present, to give a very definite reply, though the mere name of Lewis Boss is sufficient indication of the kind of work and sufficient guarantee of its thoroughness. But a recent series of remarkable papers in the *Astronomical Journal* has told us how he is correlating our fundamental star catalogues, and extracting a standard homogeneous system. These papers are, I am glad to say, collected and bound together, and will shortly be distributed; I was just in time to become the proud possessor of the first copy issued. But though there is a vast amount of information in these papers, it is so much condensed that unless they are read with extreme care, they will scarcely convey any idea of the size of the work; and it is only by visiting the observatory itself and seeing the machinery, that one can properly appreciate the true magnitude of the enterprise. The rough material is a huge card catalogue of every star down to the seventh magnitude which has been observed on the meridian. Everyone in the observatory knows that the first thing to be done in case of fire is to rush to the card catalogue and hand the boxes through the window; and at

present, thanks to the Carnegie Institution, there are a good number of people in the observatory all busy computing and checking with noble enthusiasm. I have seen something of computing at different times, but I never saw it better organized or its monotony treated more lightly. It should perhaps be stated that the computers are chiefly ladies.

THIS is scarcely the place to give any account of the manner in which this mass of accumulated material is being treated so as to extract the essence, but I will venture to allude to a couple of points. They are both taken from the printed papers above mentioned, and have therefore no novelty for those who have properly read those papers. But we are all so busy about our own affairs in these days, and have so little time for careful reading, that the most important remarks may be printed and yet pass unnoticed. And again, it is easy to read a paper without realizing the points on which the writer intends to lay chief emphasis, while in a few words of actual conversation he can convey this impression much more easily. The spoken word is in such respects superior to the printed, and from this point of view it must be admitted that an instrument called a telephone, much used in this go-ahead country, has some advantages: I fear it may come to be more generally adopted.

BUT to return to Prof. Boss's papers. In *A. J.* No. 540-1, on page 120, there is an instructive table of the necessary corrections to the declinations of various pairs of catalogues of nearly the same date, one member of each pair being made at a northern observatory, the other at a southern. Until my attention was specially directed to it, I had failed to realize the true inwardness of the table, and will venture to recommend to others interested a careful study of the argument in the adjacent text. The simple assumption is made of an alteration of the coefficient of refraction at each observatory, and is found to give excellent results. There is an appearance of arbitrariness about this assumption, for there are other known sources of declination errors; but this objection is considered and answered. If it be asked why the refractions should vary from one observatory to another, or from one epoch to another, one reply is that a systematic error of $0^{\circ}.1$ in reading or exposing the exterior thermometer is only too easy to make, and would explain many of the differences.

BUT if anyone objects that this is an old story he is altogether in the right. I hasten to make the concession and to pass to the second point by quoting from the same page the following sentence:—

It seems to me that the extension of Dr. CHANDLER's reduction of POND's declinations to include all of the zenith-distances observed by POND (1825-1835) would be a work surpassing in importance any other of this nature which could be undertaken.

Is it petty or proper national feeling which prompts the hope that this work may be undertaken in England, where Pond lived, and if possible at the Royal Observatory, where he did his work? America has already played a sufficiently conspicuous part in pointing out the merits of Pond's work, thanks to Chandler and Boss, and it would seem that the time has come for us to take over and complete his rehabilitation.

OUR last view of Niagara was on a day when the wind blew the spray right over our heads, so that we saw a splendid rainbow (or, as a small friend of mine calls it, a "sunstroke") stretching from one fall to the other. The sight reminded me of a story told the other day concerning a pious gentleman who pointed to the rainbow and asked triumphantly: "Who that sees that perfect arch can doubt for one moment that the Earth is round?"

Chicago, August 2-10.

THE Yerkes Observatory of the University of Chicago is conveniently situated some 80 miles away from the University, and is close to Lake Geneva—a little lake about 11 miles long which it is difficult to find on the map. Between the observatory grounds and the lake is a belt of virgin forest, which contrasts finely with this latest outcome of modern civilization. Beyond the forest an even more remote—a prehistoric—state of things is represented by the habitations of the "lake-dwellers." These poor folk suffer almost incredible hardships; our cruel modern regime has abolished their pile structures and driven them to build beautiful villas on the shores, and it is practically impossible for them to reach their homes without maintaining expensive electric launches. At Williams Bay Station one may see sometimes as many as 30 or 40 of these vessels waiting to receive the "lake-dwellers" who will arrive on the afternoon train from Chicago. But they are a most amiable people, and extremely hospitable, especially to strangers; and indeed I will go so far as to say that our visits to some of them were so pleasant that it is quite difficult to think of any way in which they could have been bettered. They are proud of the great observatory, and expert in finding little ways of befriending both it and the University generally; sometimes by the ordinary methods of hospitality, at others by humble little gifts of a million dollars or so, which are accepted with the most unfailing courtesy. Just recently the observatory itself has not been receiving any of the little attentions of the second class, but I feel sure that the cessation is quite accidental and temporary, and when the shy gifts are again tendered, they will be accepted just as amiably as ever, with no betrayal on the part of the recipients that they attached any significance to the discontinuity.

THERE may be some who will question my identification of these "lake-dwellers" of the present with those of the distant past.

But this is a wonderful country; look at this cutting from this morning's paper (*Chicago Daily Tribune* for August 5):—

CLIFF DWELLERS STILL EXIST.

Archæologist Discovers a Strange Race in the Sierra Madre Mountains of Northern Mexico.

EL PASO, TEX., Aug. 4.—Dr. Bush, an archæologist, just returned from a trip of 700 miles into the Sierra Madre mountains of northern Mexico, claims to have discovered a race of people that prove the perpetuation of the race of cliff dwellers. Their language, hieroglyphics, and mummies prove a connection with the prehistoric race, he says.

I don't insist upon the argument from analogy: our friends the "lake-dwellers" had no mummies to prove identity, but it is an undoubted fact that some of them have archæological tastes, and that they do not put forward their own genealogical trees may be merely due to innate modesty.

ROUND the Observatory itself, typifying the approach to the highest modern culture, is a golf-links. There are only six holes, and not many bunkers, but still it is real golf; and after all, the links adjacent to the Royal Observatory at Greenwich has only *seven* holes. It is immensely creditable to this new-born observatory that, while finding it necessary to depart in so many ways from the precedents set at Greenwich—while getting their time, for instance, transmitted to them by telephone, and keeping their transit-circle in a beautiful glass case—they should have retained the one essential feature of having a golf-links close by. Here at 5 o'clock every day in the summer everyone plays golf (in the winter they use toboggans instead), and we were fortunate enough to arrive in time to see the final round of the July tournament—a fine struggle which was carried to the 17th hole, where the Engineer-in-charge-of-the-40-inch failed with his putt.

ONE cannot help wishing that the golf-links was used a little more. They work too hard at the Yerkes Observatory, especially some of them. Morning, afternoon, and night the work seems to go on continuously, with only that hour from five to six as an intermission. The centre of it all is the 40-inch, which never rests. Every night it is someone's turn to be on duty, and the duty lasts the whole night, from sunset to dawn—rather a long stretch for a single observer, even in the summer, and in the long winter nights, with the temperature 10° or 20° below zero, the strain must be excessive. At dawn or soon afterwards the spectro-heliograph is attached, and the big telescope begins a new set of labours as soon as the Sun is sufficiently high. Nor is the work on the Sun a mere incident; when photographs have been taken on one line of the spectrum, the setting is changed to another. Then the prisms are taken out and a grating put in for still different work. The facility with which the giant telescope is dressed for the

numerous different parts which it plays during the 24 hours is one of the most striking features of the drama. The whole performance is splendid, and strikes awe into the beholder if he happens to come from lands where folk still retain the mistaken idea that one ought to rest every now and then.

WHAT a vast domain is that of Astronomy! The Harvard, Dudley, and Yerkes Observatories each represent considerable territories, all included in the domain, and yet scarcely touching one another! The capital of the first is a collection of photographs; of the second a card catalogue of star-positions; and of the third a great telescope.

NEXT to the great 40-inch, the most characteristic feature of the Yerkes Observatory is the spacious basement devoted to mechanical and laboratory work of all kinds. Here mirrors are ground and tested—one sees the huge tools for the grinding and polishing, and the long corridors for the testing; here there are lathes and machines for the making of any instrumental devices or furniture fittings required throughout the building; and here, too, is a fine concave grating with all sorts of electrical apparatus for giving arc and spark spectra. The “power-house” for running all the motors and giving light is placed at a distance from the observatory (though not from some of the dwellings, unfortunately). All this mechanical plant naturally needs considerable superintendence, and it was an excellent move to appoint recently the aforesaid “Engineer-in-charge-of-the-40-inch.” His duties are arduous, for not only does he keep all the mechanism in order during the day, but he works it at night, all night and every night, thus relieving the observer in a most valuable way. Emphatically his office is a noble and useful one, and I will adventure the hope that the present holder may long retain it, and may have better luck next time at the 17th hole.

SATURDAY is Visitors’ Day at the Observatory, and very keen the visitors are to see it. Five thousand of them put in an appearance during the year. They are not admitted until the afternoon, but they take their places at the doors as early as 11 A.M. sometimes, and wait as patiently as our crowds at the doors of a London theatre. And when admitted they try their best to ask such questions as may show an intelligent interest in what they see. “What effect have earthquakes on your instruments?” one of them will venture. “Why do you have the power-house so far away from the telescope?” asks another. “Is it because you are afraid an explosion will hurt the \$60,000 lens?” But the astronomer was ready with his answer this time; he pointed out that if he were seriously afraid of an explosion he would scarcely have that same power-house so near his \$60,000 baby.

I HAVE just been making a few experiments and calculations. In the grass round the observatory are many grasshoppers; every time one sets down a foot there jump up 20·32 grasshoppers on the average. The mean arc described by each is 2·16 feet approximately, giving a total jump of 43·8912 feet. [As it is not necessary to be quite exact we may cut off the last two decimals perhaps.] Further, let us say that 20 people go to the observatory to and fro three times a day, taking on the average 200 steps each way, 24,000 steps in all. The total induced grasshopper-jump is nearly 200 miles, if I make no mistake. If such jumps could all be cumulatively expended in approaching the Moon, even then it would take the energetic staff of the Yerkes Observatory *three years* to encourage this noble army of grasshoppers to complete the distance. This will give some notion of the enormous magnitude of astronomical distances, and the illustration is at the service of any lecturer who will use it in the true interests of education. Similar calculations might perhaps be made on the little gophers which run about the golf-links (there is obvious material for a pun here, if care be exercised in pronunciation), but I must leave that research to some one else.

THE following facts about Chicago may not be generally known: that there are public and free lawn-tennis courts and golf-links, on which anyone may play without payment; that ladies often ride astride, and very graceful they look; that there was a World's Fair here some years ago, which has left behind a number of fine buildings and open spaces, but that one of the less desirable relics was the dumping on the city of some thousands of people who were found stranded after the Fair had gone, and were not re-exported without immense labour and expenditure; that there is a "meat-strike" going on just now, which has already doubled the price of beef; that the children play on the shores of Lake Michigan and paddle in its waters just as though they were at the seaside, and if only the scent of the salt could be imported the illusion would be perfect; that the city draws an inexhaustible water-supply from points in the lake some five or six miles out from the shore by underground channels.

WITH regard to the water-supply, the precaution of going five or six miles out does not always ensure that the water will be satisfactorily pure. Careful tests are made continually, and with certain winds it is found that the percentage of organic matter sensibly increases; but this is duly notified in the morning papers in order that the water may be boiled before use. In the words of an American friend, "They examine the water for bugs, and when they find any they advise you to fry them."

THE morning papers differ in many respects from our own, but we are learning our way about them, and feel a pleasurable interest in some new items, when there is no very original lie from Che-Foo

about the war. Thus it is remarkable to note that whereas when we were in Boston the local baseball team was at the head of the American League, now that we have come to Chicago the championship honours have come thither also. We think of asking Mr. Rider Haggard whether there is any trace of possible telepathic influence here. Another item we scan daily is the account of the weather at St. Louis, where we have to go in September, and where we are told it will be warm. But the accounts are sometimes puzzling, as, for instance, the following from the *Chicago Tribune* of August 10:—

WEATHER IN ST. LOUIS YESTERDAY.

Max. 80, mean 84 ; wind, S. ; precipitation, 0 ; fair.

Is it "fair" for the mean to be higher than the maximum ? Then there is the news about the Presidential Campaign. I have just been reading the manifesto issued by Judge Parker, who has accepted nomination as Democratic Candidate, and the following sentence strikes me as worthy of note :—

The display of great military armaments may please the eye, and, for the moment, excite the pride of the citizen, but it cannot bring to the country the brains, brawn, and muscle of a single immigrant, nor induce the investment here of a dollar of capital.

THESE references to life in Chicago arise out of the necessity of coming into the city to give some lectures to the "Summer School" of the University. The Chicago University, like many other Universities in America, is busy all through the year, for though many of the regular students are away during the summer, the doors are then thrown open to what we should call "extension" students. These are generally people engaged in teaching through the country, who seize the opportunity to learn something in their hard-earned vacations, and most interesting folk they are. To Chicago come many from the West, ready to pay their fees and lodging by any kind of honest labour in the hours when they are not at lectures—lighting the street lamps or waiting on the richer students at table. It is a most impressive experience to see and talk to them ; but only one of a whole series of such impressions which crowd on the attention in this energetic city. To contemplate the University of Chicago itself is to fill one with wonder : only eleven years old, and such an infant for its age ! I saw it eight years ago and admired it then ; but to see it now is almost bewildering. Buildings of a magnificent type have shot up like mushrooms, and testify to great liberality, not only of purse but of view. In new cities we are prepared for fine laboratories, such as the Ryerson Physical Laboratory and the Kent Chemical Laboratory, but a new chord is struck when we see reproductions of Christ Church Hall and Magdalen Tower alongside them. We started out on a tour of inspection, but gave it up when confronted with the temptation to linger in the Physical Laboratory instead. Here Prof. Michelson has set himself to make a screw—something worthy the name of a screw, with no error greater than the

millionth of an inch, and the research is fascinating. What a critical moment that was in scientific advance when Rutherford tried to measure his early astronomical photographs and found that he must have a better screw, and so set out to make one! His early successes led him to rule gratings, and then to improve the gratings the screw had to be improved still further; and so came Rowland's work, and now Michelson's work, and still we are as anxious as Rutherford was for a better screw. Meantime, astronomical photographs are being measured without a screw or with screws of which comparatively little is expected.

BUT to return to the Yerkes Observatory (as, in fact, we did more than once, and with great delight, in spite of the many attractions of Chicago). I feel that I have, perhaps, scarcely said much about the actual work going on. But after all this can be gathered from other sources. On one or two nights I enjoyed the great privilege of a few peeps through the 40-inch; we looked for the ninth satellite of Saturn, but without success; at some of the clusters, which look magnificent in the giant telescope; and at some of the planetary nebulae, which really show some resemblance to those drawings that many of us have found so difficult to reconcile with views in smaller instruments. There is no doubt at all about the difference in focus for these nebulae and for stars, to which Prof. Barnard has drawn attention; not even the most casual observation could miss it.

A PRETTY piece of apparatus in the Yerkes Observatory is that for projecting the spectroheliograms into a globe ruled with lines of latitude and longitude, whereby the positions of spots and flocculi can be read off in heliographic longitude and latitude direct to $0^{\circ}.1$, thus saving all computations. The globe is of iron, and was carefully turned and ruled in the Observatory workshop. The projecting apparatus in use is rather on a magnificent scale, including a 12-inch lens and a 12-inch mirror, and if these are essential (which I have not had opportunity to consider), the method might be beyond the resources of a small observatory. But viewed as a capital expense which saves an enormous amount of computation, even these are really trifling. I took some pains to look into the details of working, and can recommend the plan strongly to other observatories doing similar work.

BUT all good times have an end, and there comes a day when we must start for the Wild West, leaving Chicago and its University and its semi-detached Observatory. We get the kindest of words and hand-shakes from the hard-working devoted little community; we breakfast for the last time with our eyes on that view of the lake; we drive down to the station to find the steam-launches delivering up the "lake-dwellers" to the morning train; we spend a last day in the busy hum of Chicago; and then hey! for a merry three days in the train.

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EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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No. 350.

Some Reflections suggested by the Application of Photography to Astronomical Research.*

THE European astronomers here present have to thank the organizers of this Congress for much more than their hospitable invitation to attend it, and the opportunities thus afforded of meeting here in St. Louis so many men eminent in their own or other branches of knowledge: over and above this they owe to them opportunities of seeing the great observatories which have developed so rapidly in this country during the last quarter of a century, and of admiring at close view the resources and the work of which the fame had already reached us across the Atlantic. This is not the time or the place for any account of what we have seen and learnt; but not to put on record a word or two of appreciation of the great works accomplished, and of that munificence on the part of American citizens which has rendered them possible, would be indeed an omission. We from Europe are, in at least one respect, critics well qualified to judge whether an adequate return is being obtained for endowments such as have recently fallen to the happy lot of American astronomers, for most of us have had some practice in the use of such endowments—*hypothetically*. The constraints of more modest equipments have inevitably suggested plans for work on a larger scale—observatories-in-the-air which our imaginations fill with beautiful and novel apparatus, where the preliminary trials are always successful and no mistakes are made. We come to you accordingly prepared to judge what we see by comparison with a very high standard, and you may well be content with the commendation which we offer unstinted. We

* Address delivered by H. H. Turner, D.Sc., F.R.S., Savilian Professor of Astronomy in the University of Oxford, in the Section of Astrophysics at the Congress of Arts and Sciences at St. Louis, on Wednesday, September 21, 1904, at 4 P.M.

rejoice to think that, in the presence of the new and vast possibilities opened up by the gradual accumulation of facts during the last century, by the invention of the spectroscope, and by that of the photographic plate, Astronomy should be so fortunate as to receive valuable aid just at a time when it is so urgently needed. It may be well for us to glance for a moment on the other side of the picture, and to wonder what would have been the course of events if this timely aid had not come. How would Astrophysics, the new-born child of Astronomy, have been nourished? We can scarcely think that it would have been allowed to want for nutriment, but whatever was given to it must inevitably have been withdrawn from the scanty stock of the parent science; either parent or child, if not both, must have shown signs of starvation. This danger is by no means entirely averted even yet; the needs of both, especially of the youthful Astrophysics, are increasing daily, as in the case of any other young and healthy organism. The future is not free from anxiety; but that the present is not actually a time of distress is largely due to the generosity displayed towards our science on this side of the Atlantic.

I am tempted to make a remark regarding another science, suggested by the above considerations in conjunction with incidents of travel. No one can cross this great continent and note the extraordinarily rapid spread of civilization, without feeling his interest drawn forcibly to the remnants of the former state of things; to the few remaining native tribes and the monuments of their ancestors scattered through the land. No man of science, whatever his main interest may be, can be insensible to the vital importance of securing permanent records of these vestiges before they inevitably perish. No astronomer who is properly grateful for the endowment of his own science in time of need can fail to hope that the science of Anthropology may be equally fortunate at a most critical juncture. I have not the means of knowing whether the vanishing opportunities are being properly cared for: I earnestly hope it may be so; but, if it is not, surely this great assembly of men from all sciences and nations could not unite to better purpose than to urge on the American nation the supreme importance of special assistance to Anthropology at the present time. We all have needs, even pressing needs, but the pressure is not usually of this kind. The subject-matter of our investigations is not evanescent; we astronomers, for instance, know that if we must perforce put aside a particular investigation for lack of means, fifty years hence a more fortunate successor will find the eternal heavens little changed for the same purpose. But the anthropologist cannot wait; with him it is now or never, and Science would be a poor thing indeed if we could not be so unselfish as to recognize his needs as more urgent than our own. Is it too much to hope that, even before we leave this hospitable city, we may have some assurance that full justice shall be done in this matter?

It is a familiar fact that there are epochs in the history of a science when it acquires new vigour; when new branches are put forth and old branches bud afresh or blossom more plentifully. The vivifying cause is generally to be found either in the majestic form of the discovery of a new law of Nature, or in the humbler guise of the invention of a new instrument of research. The history of Astronomy has been rich in such epochs, notable among them being that when Newton announced to the world the great Law of Gravitation, and that when Galileo first turned his telescope to the skies.

We have within the last half-century been fortunate enough to include another great epoch in astronomical history, characterized by the birth, almost a twin-birth, of two new scientific weapons—the spectroscope and the sensitive film. It is, of course, somewhat difficult and scarcely necessary to assign an exact date for the origin of either of these; the spectroscope was perhaps first systematically used on the heavenly bodies by Huggins, Rutherford, and Secchi in the 50's, but we may trace it back to the early work of Fraunhofer, who described the spectrum of Sirius in 1817, or further back to the experiments of Newton with a prism; and the *dry-plate*, which in particular has conferred such benefits on our science, had of course its precursors in the collodion plate or the daguerreotype. But the greater part of the influence on astronomy of both the spectroscope and the photographic method dates from the time when the dry plate was first used successfully, not much more than a quarter of a century ago; and in that quarter of a century there have been compressed new advances in our knowledge which perhaps will compare favourably with the work of any similar period in centuries either past or to come. It is difficult to estimate at their true value historical events in which we play a part, and any review of such a period undertaken now must be necessarily imperfect, for we are advancing so rapidly that our point of view is continually changing. But it is an encouraging thought that obvious difficulties may enhance interest in the attempt and suggest kindly excuses for its shortcomings.

From the embarrassingly large number of possible topics which the period provides, I have selected that of astronomical photography, and I invite your attention to some characteristic features of the photographic method in astronomy, and some reflections thereupon. It is scarcely possible to avoid repeating much that has been said already, but I hope it will be clear that no claim to originality is advanced; in what follows I wish to claim nothing as mine save its imperfections.

The advantages of the photographic method, which attracted attention from the first, may be grouped under three heads—its power, its facility, and its accuracy. The lines of demarcation are ill-defined, but the classification will help us a little, and I proceed to consider the groups in this order.

The immense *power* of the photographic method as compared with the eye arises from the two facts that (a) by the accumulation of long exposures fainter and fainter objects can be detected, and that (b) large regions of the heavens can be recorded at the same exposure. No property of the photographic plate has excited more marvel than the former—that it can detect objects too faint to be seen even by our largest telescopes; objects of whose very existence we were in ignorance and should have remained in ignorance. Early successes have been followed up by others more striking as years have rolled on, as better instruments have been devised, and the patience of the watchers has proved equal to greater strain. It is here that the change from the “wet” plate to the “dry” has proved most advantageous. The possibilities with the former were limited to the period during which it would remain wet; with the latter, exposures may be continued for hours, days, even years—not, of course, continuously in the case of astronomical photography, for the camera must be closed when daylight approaches; but it can be opened again at nightfall and the exposure resumed without fault. In this way objects of extraordinary faintness have been revealed to us. When Nova Persei had flashed into brilliance in 1901, and then slowly faded, long-exposure photographs of its region revealed to us a faint nebulous structure which we could never have seen; they told us that this structure was changing in appearance in a manner which it taxed our ingenuity to explain, and about which speculation is still rife. But a greater triumph was to come; even the *spectrum* of this faint object has been photographed. When we consider that in the spectrum each point of light in the object is enormously diluted by being spread out into a line, the difficulty of this undertaking seemed almost prohibitive; but it was not sufficient to prevent Mr. Perrine, of the Lick Observatory, from making the attempt, and he was deservedly rewarded by success. I may be wrong in regarding this success as the high-water mark in this direction at the present time, and it will probably be surpassed by some new achievement very shortly; but it will serve to illustrate the power of photography in dealing with faint objects.

But may we here pause for one moment to marvel at the sensitiveness of the human eye, which is such that it is, after all, not left very far behind in the race? The eye, sensitive as it is merely to transient impressions, is no match ultimately for the plate, which can act by accumulation. But with similar instruments the plate must be exposed for minutes or even hours to seize the impression of a faint object which the eye can detect at a glance. There seems to be no reason in the nature of things why the eye should not have been surpassed in a few seconds; and in the future the sensitiveness of plates may be increased so that this will actually be the case, even as in the past there was a time when the sensitiveness was so small that the longest exposure could not compete with the eye. But this time is not yet come, and at the present

moment the eye is still in some departments superior to its rival, owing to this very fact, that though it can only see by glances, it can use these glances to good effect. In the study of the planets the more clumsy method of the photographic plate (which, by requiring time for the formation of the image, confuses good moments with bad) renders it almost useless as compared with the eye; and again, we have not as yet used photography for daylight observations of stars.

But there is another direction in which the photographic plate is immensely superior to the eye in power; it can record so much more at once*. In the able hands of Prof. Barnard, Dr. Max Wolf, and others, this property of the plate has been used to record the presence in the sky of vast regions of nebulousity such as, we may safely say, the eye would never have satisfactorily portrayed, not altogether because of their faintness (for in one of his papers Prof. Barnard tells us that he was actually led to photograph such a region because he had become vaguely conscious of it by eye-observation), but because of their diffusion. It is noteworthy that these beautiful photographs were taken with comparatively humble instruments, and we may be as yet only on the threshold of revelations still to be made in this direction.

Secondly, the photographic method represents a great advance in facility of manipulation. A familiar example may be taken from the domain of planetary discovery. In old time, to recognize a new object among numerous fixed stars, it was necessary either laboriously to map out the whole region, or to learn it by heart, so that it was practically mapped in the brain. Now all this labour is avoided; two photographs of the same region, taken without any strain on the memory or the measuring ability of the observer, can at a glance, by a simple comparison, give the information that a strange object is or is not present—information formerly obtained at so much cost. Sometimes, indeed, the cost was so great that the information was not obtained at all. For 15 years Hencke searched without success for a planet, and for nearly 40 years after the discovery of the first four small planets in 1807 no further discoveries were made, though hundreds were constantly crossing the sky, and a dozen new planets are now found every year with little trouble.

* This property has been beautifully illustrated by a lecture-experiment of Prof. Barnard. He throws on the screen a picture of a large nebula which the photographic plate has no difficulty in portraying all at once; but the picture is in the first instance covered up by a screen, except for a small aperture only, and this aperture, he tells his audience, represents all that can be seen by the eye at one time, using the giant telescope of the Yerkes Observatory. By moving the screen about, different portions of the picture may be viewed successively, as also by moving the telescope about in looking at the sky itself. But what a revelation follows when the screen is removed and the full glory of the nebula is exhibited at a single glance! We can well understand that the true character of these objects was hopelessly misinterpreted by the eye using the imperfect method of piecemeal observation which alone was formerly possible.

But though this instance of increase in facility is striking, it is far from being the only one or even the most important. Wherever we require a record of any kind, whether it be of the configuration of stars, or of solar spots, or of the surface of the Moon, or of a spectrum, the labour of obtaining it has been enormously reduced by the photographic method. Think for a moment of what this means in the last instance only—think of the labour involved in mapping one single spectrum by eye-observation; of the difficulty of settling by such a method any doubtful question of the identity of certain lines in the spectrum of a star. A few years ago Dr. McClean announced that he had found oxygen in the star β Crucis. Up to that time this element, so familiar to us on this Earth, had appeared to belong to us alone in the Universe, for in no spectrum had its lines been detected. The proof of its existence in β Crucis depended on the identity of a number of lines in the spectrum with some of those of oxygen; and the measures were sufficiently difficult on a photograph, so that for more than a year the scientific world refused to pronounce a verdict. How long would the case have dragged on if only visual measures had been possible? We may fairly doubt whether a definite conclusion would ever have been reached at all. By the sheer facility of the new method of work we have advanced by leaps and bounds where we could only crawl before.

Thirdly, there has been a great gain in *accuracy* from the introduction of photography; and it is this quality which is above all of value in the science of Astronomy*. The wonderful exactness of the photographic record may perhaps best be characterized by saying that it has revealed the deficiencies of all our other astronomical apparatus—object-glasses and prisms, clocks, even the observer himself.

It has almost been forgotten that in the early days the accuracy of a photograph was doubted. Even now it can scarcely be said that we know definitely the stage of refinement at which we must begin to expect irregular displacements of the images from distortion of the photographic film; but we have learnt that they do not occur in a gross degree, and that other apparatus must be improved before we need turn our attention seriously to errors arising from such a cause. Consider, for instance, what photography has told us about our optical apparatus, which we regard as having reached a high stage of perfection. We are accustomed to think of properly made optical apparatus as being sufficiently similar in all its parts; it is tacitly assumed in the principle of

* Two things may be measured on a photographic plate—the position of an object, or the density of the image; the former being an indication of its position in the heavens, and the latter of its brightness. With the latter topic I do not propose to deal, for the reason that it is in the hands of a much abler and more experienced exponent; but the former alone will provide enough food for reflection.

the heliometer, for example, that one half of the object-glass is sufficiently similar to the other. But a stock adjustment recently adopted in photographing a spectrum for accurate measurement exhibits clearly the errors of this assumption. Photographs are taken of the spectrum through the two halves of the objective; and if they were properly similar the lines in the two halves of the spectrum should fit exactly. A mere glance is usually sufficient to show discordances. It is true that one of the photographs is taken through the thick half of the prism and the other through the thin, so that errors of the prism are included; but these, again, are optical errors. They are, however, not the only sources of error which at present mask photographic imperfections. Glass plates are not flat, and this want of flatness introduces sensible errors. Even with the great improvements in our driving-clocks which were called for immediately photographs were to be taken—with electrical control and careful watching on the part of the observer,—there is apt to creep in a “driving-error” which gives bright stars a spurious displacement relatively to faint. We must get flatter plates, better driving-clocks, and watch more carefully before we can certainly accuse our photographs of a failure in accuracy. Nevertheless, there are indications that we may be near the limit of accuracy even now. Examination of the réseau lines on various plates appears to show small displacements for which no cause has yet been assigned; and the end of our tether may not be far away. But as yet we have not been pulled up short, and there is hope that the warning may be, as on one or two previous occasions, a false alarm.

Such being the accuracy of the photographic method, it is surprising that it should not as yet have been more fully adopted in that field of work where accuracy is of the greatest importance—namely, in what is called fundamental work, with the transit-circle or other meridian instruments. The adoption of new methods is always a slow process and there are at least two classes of difficulties which hinder it. The first class has its origin in the instinctive conservatism of human nature, wherein men of science differ little from their fellows. The second has to do with available capital; and in this respect we are distinctly at a disadvantage compared with other men; for when a new instrument of *general* utility is invented, at once a large amount of capital is invested in working out the details and improving them to the utmost, whereas for a scientific instrument no such funds are available. Think, for instance, of the money spent in perfecting the bicycle, and the time occupied in developing it from the earliest forms to those with which we are now familiar—from the “boneshaker” of the sixties through the high bicycle which we saw 20 years ago, to the modern machine. Think, too, how totally unexpected have been some of the incidents in the history of this machine—such as the introduction of pneumatic tyres, or its use

by ladies *. In the case of such an instrument, now universally adopted, if rapid development could have been secured by expenditure of money and brains, surely enough of both commodities were forthcoming to attain that end; and yet simplicity and finality have probably not yet been attained in a period of 30 years. When we compare the small amount of money and especially the small number of persons that can be devoted to the perfection of a new scientific method, such as the use of photography in astronomy, it will excite little surprise that progress during the same period of 30 years has been slower. In commerce old machines can be thrown on the scrap-heap when improvements suggest themselves; but who can afford to throw away an old transit-circle? The very fact that it has been in use for many years renders its continued use in each succeeding year the more important from considerations of continuity.

It is doubtless for such reasons as these that little has yet been done in the way of utilizing photography for meridian observation. Although one or two meritorious beginnings have been made, which have sufficed to show that there are no insuperable difficulties in the way, up to the present moment no meridian instrument of repute is in regular work using the photographic method. And this fact cannot, after all, be completely explained by the reasons above mentioned. Opportunities for setting up costly new instruments do not occur frequently in astronomy, but they do occur. In the last decade, for instance, large transit-circles have been set up both at Greenwich and the Cape of Good Hope; but in neither instance has any attempt been made to adopt the photographic method. The Washington Observatory was reconstructed well within the period since the great advantages of photography have been recognized; and yet not even in the United States, the land of enterprise, was a start then made in a direction in which it is certain that we must some day travel. That day has probably been deferred by the stimulation of competing methods which a new one brings with it. When electric light was first introduced into England, the gas companies, stimulated by the stress of competition, adopted a new and improved form of light (the incandescent gas) which put them at a much less serious disadvantage compared with their new rival. So when photography began to show what new accuracy was attainable in measurement of star-positions, it would almost seem as if the devotees of the older visual methods were compelled to improve their apparatus in order not to be left wholly behind in the race. The Registering Micrometer † was produced by Messrs. Repsold, with the astonishing result that the troubles from personal

* I have in my possession a copy of a work of reference on cycling, dated no earlier than 1887, in which it is carefully stated as a deliberate conclusion that ladies will never use the machine to any great extent.

† We have been accustomed hitherto to determine the position of a star by observing the instant when it crossed a fixed wire; but it has long been known

equation, which have so long been a difficulty in all fundamental work, have practically disappeared.

This beautiful invention has placed the eye once more in a position actually superior to the photographic plate; for with the eye we can observe stars in daylight, and so secure information of great importance, whereas no photographic method of doing this has, as yet, been devised. And there is also the fact that for faint stars a long exposure would be required for what the eye can accomplish in a few seconds.

Thus in one or two astronomical channels the effects of the rising tide of photography have scarcely yet been felt; but into all the others it has swept with ever growing force. Looking back over the 30 years of advance, we may be well satisfied. With more funds, and especially with more men, no doubt more could have been done: let us even admit that we might have done better with the same funds and the same limited staff. But on the whole we have been fortunate. At a critical time, when we might have felt the want of larger endowments acutely, the need was almost anticipated by a stream of benefaction. If this stream had its chief source in the United States, its beneficial effects have poured over the whole world; and induced currents have begun to flow elsewhere. We may reflect with thankfulness how much harder our advance might have been but for the noble gifts to the Harvard, the Lick, and the Yerkes Observatories: and earnestly hope that the cheerful expectations of a great American astronomer, that these are but the foreshadowing of much larger gifts to science, may be adequately realized.

[To be continued.]

that two different observers record systematically different instants—they have a personal equation. Recently we have learnt that this personal equation varies with the brightness of the star observed, and with other circumstances, and to make the proper corrections for it has severely taxed our ingenuity and involved much work. Before the invention of photography, we might well bear this with patience, since it seemed to be inevitable; but the photographic plate, which is free from human errors, offers a way of escape from all troubles—at the expense, no doubt, of some little experimenting, but with every prospect of speedy success. Eye observation, which had borne this burden so long, must get rid of it if it was to march alongside the untrammelled photographic method; and the surprising thing is that it has actually done so. The adopted device is extremely simple: replace the fixed wire which the star crosses by a wire which moves with the star and registers its own movements. The registering is done automatically; but the motion of the wire is controlled by the observer and there is still room for a new form of personal equation in this human control. But none manifests itself, probably for the reason that we no longer have two senses concerned, but only one. In recording the instant when a star crosses a wire we employ either the eye and the ear, or the eye and the sense of touch; and personal equation arises from the different co-ordination of the two senses in different people. But in making the wire follow the star, the eye alone is concerned, and there is no longer any room for difference in “latent period” or other co-ordination of two senses.

Evening Meeting of the Royal Astronomical Society.

A MEETING of the R. A. S., additional to the ordinary series, similar to two that were held last Session, took place on Friday, October 28, at 8^h 30^m P.M. The lecture was delivered by Mr. T. Lewis, on Double Stars, and after a word or two of introduction by the President, Prof. H. H. Turner, he began by sketching out the early history of double-star observations, showing how Sir W. Herschel, in search of pairs for determinations of parallax, was finally led to the discovery that binary systems existed amongst the stars; how M. Struve took up the work in a systematic and thorough manner, and produced the '*Mensuræ Micrometricæ*' and the '*Positiones Mediae*.' These two huge volumes contained respectively the micrometer measures of 2640 stars and the meridian observations determining their places. He then paid a tribute to the conscientiousness and perseverance of Dembowski, and finally brought up the history of double stars to Burnham and the present observers at Lick. Pointing out the small interest taken in this branch of astronomy amongst English amateurs, he showed the utter fallacy of supposing there was no room for small instruments, and brought forward as proof the splendid work done with a 6-inch refractor by Burnham, who actually discovered, with this small instrument, 15 pairs under 0".5 separation, and 60 pairs between 0".5 and 1".0 separation. Further, he has been fully repaid by having amongst these:—

β 151,	a binary with a period of 27 years.
β 101,	" " " 22 "
β 716,	" " " 33 "

He then described the backward state in which the discussion of the observations was in at the close of the last century and pointed out how the leeway had been in a great measure made up by Prof. Burnham, who had collected and discussed his 1290 stars in Volume I. of the Yerkes publications, and also by Prof. Hussey, who had done similar work for the 514 Otto Struve pairs in Volume V. of the Lick publications, and that the Council of the Society were now printing a discussion of the 2640 pairs in Struve's '*Mensuræ Micrometricæ*.' He then threw on the screen diagrams illustrating the apparent, true, and absolute orbits of γ Virginis, ξ Ursæ Majoris, Castor, and stated that the components of γ Virginis were practically equal in mass, that the masses of α^1 and α^2 Geminorum were in the proportion of 4 to 47, and that the mass of the cones of ξ Ursæ Majoris was greater than the mass of the principal star in the ratio of 3 to 2. Other systems were described including ζ Cancri. He then showed how, by a combination of micrometer measures with meridian observations, the proper motions of many faint stars could be determined, and incidentally showed how the two methods of observation were becoming more and more interdependent. Finally curves were

exhibited showing the distribution of the Struve double stars in R.A., and also the relation of the two classes—those relatively fixed and those showing relative motion. This latter curve he suggested might be interpreted in such a way as to give an idea of the shape of the Stellar Universe as projected on the plane of the Equator, and also the Earth's position on this plane.

The lecture was received with much appreciation, and at its close, Prof. Turner made some remarks in eulogy of Prof. Burnham's work and described an interview that he had with that gentleman during his recent visit to America. A hearty vote of thanks was accorded to Mr. Lewis.

Prof. Turner then announced that Mrs. Isaac Roberts had given a series of her late husband's astronomical photographs to the Society. Mr. W. S. Franks exhibited some specimens of these and made some remarks about the spiral nebulae.

The President then called attention to the fact that the rooms of the Society had been redecorated during the summer and that the system of the electric lighting was now a continuous current instead of an alternating system as before.

The Fellows, of whom a comparatively large number were present, then adjourned to the library, where Dr. Roberts's photographs and some other objects of interest were on view.

The Ninth Satellite of Saturn.

IN Harvard Annals, Vol. LIII. Part III., which has just come to hand, Prof. W. H. Pickering tells the whole story of his discovery of the outermost at present known of Saturn's moons. The search began as long ago as the year 1888, when the 13-inch Boyden telescope was used to take photographs of Saturn with exposures of an hour, but no satellite was found, and the effort appears to have been given up because it seemed that no unknown satellite could exist which was bright enough to be photographed with the telescope then available; but when the 24-inch Bruce telescope was set up in Arequipa this research was borne in mind and another series of photographs taken in the years 1897 and 1898 under more favourable circumstances. In 1899 March a careful examination of these plates was begun and, as everyone knows, on four photographs taken on 1898 August 16, 17, 18 with exposure of one or two hours a small object was detected at a considerable distance from Saturn, but moving with it, which was inferred to be a satellite. From the positions of the object it was shown that the period must be either about 4200 or 490 days, but obviously from the scanty material it was impossible to predict with much accuracy. There were, however, three more photographs available taken in 1898 September, but the images on these were blurred and not in the expected places, which last

fact, it is now known, was due to the great eccentricity of the satellite's orbit, so that it was not considered advisable to use them.

At this period it was found necessary to photograph Saturn only when he was at a stationary point of his orbit; the plan of altering the rate of the driving-clock of the telescope to compensate for the motion of the planet was not adopted until 1900, so that photographs were not taken again until 1899 August. The planet was then in the Milky Way, where the multitude of stars made the search much more difficult and an examination of the plates led to no satisfactory result. Plates were taken during the spring and early summer of 1900 and at first examination no satellite could be found on these, and Prof. Pickering "began to wonder if the four images discovered on the plates of 1898 might not after all have been defects or faint stars occurring by a curious coincidence in exactly the proper places to represent the motion of a satellite"; but a re-examination of the images showed that there was no possibility of their being defects. Then the idea occurred to Prof. Pickering to search on the plates of 1900 at a greater distance from the planet than he had hitherto done, and an object was found on one of them 33' west of Saturn which turned out to be the satellite. This brief statement of the facts of the re-discovery gives an inadequate idea of the amount of work involved, about which Prof. Pickering goes into some detail. The general procedure was to construct a map of the region in hand, copying from one of the negatives, and then each negative in turn was compared with this, the stars on the map being marked if shown on the other negatives, so that by a process of elimination the field of search was narrowed. But in the vicinity of the Milky Way about 6 stars are found for each square minute of arc, or 22,000 per square degree, so that it was necessary to construct a map of about eighty stars before beginning the search. In the earlier part of the work it required on the average about four hours to find the satellite on each plate, and sometimes the time spent was much longer. The actual measures of the satellite when found were made by placing a reticule ruled to millimetres against the negative, enlarging it five times, and the coordinates were then measured by placing a finely divided glass scale against the film of the enlargement.

Sixty photographs of Saturn had been taken up to 1902 September with the Bruce telescope, and on seventeen of these, for different reasons, it was not expected that the satellite should be shown, but of the remaining forty-three it failed to appear on only one. From the measures of the satellite on these an orbit and an ephemeris had been computed when circumstances happened to entirely alter the conclusions. Photographs were taken at Arequipa in April and May of this year (1904), and on these Phœbe could not at first be found, but on searching at some distance from the computed place, a faint object, presumably the

satellite, was seen, and it became necessary to explain why its position was not in accordance with the originally computed orbit. After several hypotheses had been tried and rejected, the suggestion occurred to Prof. Pickering that the satellite was describing an orbit in a retrograde direction, and an orbit being computed on this supposition all the observations fitted except eleven of them, which had already been marked as of doubtful value on account of faintness of the images, so this hypothesis has been accepted as the truth.

The following appear to be the data deduced from these observations :—The eccentricity of the orbit is 0.22 , the inclination of the orbit to the ecliptic is $5^{\circ}.1$, and to the orbit of Saturn $6^{\circ}.0$. The longitude of the ascending node is 220° , and of perisaturnium $289^{\circ}.7$. The epoch at which the satellite passed perisaturnium was 1900 March 28^o G.M.T. The period is 546.5 days.

This eccentricity 0.22 is greater than that of any other satellite or larger planet in the solar system and is exceeded only by a few of the asteroids. The brightness of Phœbe is estimated at two magnitudes fainter than that of Hyperion, which is assumed to be of the magnitude 14.0 . Phœbe when full would appear from Saturn of the magnitude 5.2 to 6.2 , depending on its distance from that body. This distance varies from 6,210,000 to 9,740,000 miles. From photometric considerations its diameter is thought to be about 200 miles. It is thus seen to be, exclusive of comets, the largest body discovered in the solar system since the inner satellites of Uranus were found in 1851. It is also by far the faintest object.

It may seem remarkable that one satellite of the family should revolve round the planet in the opposite direction to the other eight, but Prof. Pickering ventures an explanation. He says that in the case of any planet revolving about the Sun, if a portion of its constituents are fluid an annual tide will be produced, and the effect of this will be to change the angle of inclination of the axis of the planet to the plane of the orbit. The axes of all planets are in a state of unstable equilibrium owing to this annual tide, and they will only become stable and permanent when the planes of their equators coincide exactly with the planes of the orbits, as is now practically the case with Jupiter. The bearing that the retrograde motion of Phœbe has on this hypothesis is that at the remote time when it was formed and Saturn was a large diffuse nebulous body filling the whole orbit of the satellite, Saturn was revolving in a retrograde direction. During the ages that elapsed before the next satellite (Iapetus) came into existence Saturn had changed the plane of its rotation so that its motion was direct. Since Iapetus was formed the plane of its orbit has been drawn closer to the plane of the orbit of its primary by the same forces that have produced this effect in the case of our Moon.

The Universal Time System.

It may be comparing small things with great, but in the Far East, where many stirring things have been lately happening, a reform of a pacific nature has been in progress which, in its way, is a connection between eastern and western civilization. China has adopted, or is adopting, the Universal or Zone Time System. The alternative tense is used advisedly, for the matter is not yet complete, but it is proposed to give here the present position of affairs.

Though the map of Eastern Asia is, on account of current events, probably familiar to most persons, it may be noted that the meridian 9 hours east of Greenwich passes nearly through the centre of Japan, and that the longitude of Port Arthur is 8 hours 5 minutes east. The Standard Time 9 hours fast on Greenwich was adopted in Japan in 1896, and has since come into use in the Philippine Islands. Gradually, and without any public notification, the Standard Time 8 hours fast on Greenwich has crept into use along the coast of China from Newchang, in the north, to Swatow, nearly the southernmost point; also up the Yangtse-kiang as far as Hankow, and at Wei-hai-Wei and Tsingtau. It will be noted that, with the exception of Wei-hai-Wei, this territory is all non-British. There is an observatory at Hongkong, under the colonial government, in longitude $7^{\text{h}} 37^{\text{m}}$ east of Greenwich, and this local time is used in the colony; but it seemed good to the Hongkong Chamber of Commerce that the port, and consequently the West river ports and Canton, who use the same time, should fall into line with the rest of the country and adopt the times of the 8-hour zone. The main reason urged for the adoption was that the railway-systems in China are now being developed, and that it is better that the change should be made now, before the Hongkong lines are connected with those of the rest of China. Another point, perhaps a minor one, in favour of the change, is that, if the business time-tables remain the same there will be more daylight after office-hours than at present. The authorities at the Colonial Office having been approached by the Governor of Hongkong, gave their consent to the change of time-system, which will therefore soon be made. The Court of Directors of the British North Borneo Company, having been communicated with, expressed their willingness to join the scheme, and gave instructions for the adoption of the 8-hour zone time in British North Borneo and the island of Labuan.

The theoretical line of demarcation between the 8- and 9-hour zones, viz., the meridian $127\frac{1}{2}^{\circ}$ east of Greenwich, runs centrally through Korea. Which time they prefer to keep no doubt the Koreans will settle when other matters demand less of their attention. The 8-hour zone, the time of which, it will be remembered, has been in use in West Australia for some years, would include all China eastward of Hankow on the Yangtze-kiang and

Canton on the Chekiang, besides some territory west of these places, so that it would contain all the main railway-systems and all the Treaty Ports except the two most westerly, Pakhoi and Hoihow; but the exact line of separation is not yet defined, and for the moment these two last-mentioned places may be considered to be in the 8-hour zone.

If Western China adopts the time 7 hours fast on Greenwich as standard, this will be the first territory to use that time. No country as yet uses the times 3 hours or 4 hours fast on Greenwich. These zones contain Mauritius, Arabia, Persia, and Afghanistan and part of Russia. India and Further India are on the eve of adopting a Standard Time System, which it is hoped will cover the zones 5 hours and 6 hours. These latest links may be considered as completing the chain, and the Universal Time System in about twenty years—it was in 1886 when the Astronomer Royal gave his address on the subject at the Royal Institution—has encircled the globe. Despite Mr. Kipling's refrain that "West is West, and East is East, and never the twain shall meet," they have one thing in common—they both take their time from Greenwich.

H. P. H.

The following is part of an article on the same subject written by Signor F. Oom, of the Lisbon Observatory, for the *Revue Scientifique*, Paris :—

Il n'y a pas, en effet, une raison fondamentale pour préférer ce méridien, mais le fait est qu'il est préféré par toutes les nations, sauf la France; or ce n'est pas par humilité, par crainte, ni par désir de plaire, que les Etats-Unis, le Japon, l'Allemagne, la Russie, ont adopté pour leurs cartes géographiques et marines, pour leurs éphémérides nautiques, et pour leurs coordonnées géodésiques, le méridien anglais. Une autre raison que celle du plus fort a provoqué cette unanimité, ou plutôt c'est celle du plus fort, mais non dans le sens matériel: le plus fort dans ce cas est d'un ordre entièrement spirituel et scientifique. C'est celui qui, depuis une plus longue suite d'années, a contribué efficacement, et avec une persévérance jamais démentie, au perfectionnement de la navigation; c'est celui auquel sont rattachées directement le plus grand nombre de déterminations de longitude; c'est celui enfin qui, dans notre époque de représentation parlementaire et de vote universel, a réuni sans conteste, et sans lutte, l'imposante majorité des voix. Or la majorité (la foule, parfois criminelle, comme l'étudiait naguère dans cette *Revue* l'éminent penseur Gustave Le Bon) est aujourd'hui le pouvoir absolu, fût-elle composée de portefaix, contre l'opinion même de quelques spécialistes ou compétents. Et dans le cas du méridien de Greenwich, cette majorité qui l'a adopté est au contraire composée de spécialistes justement, car ce sont les géographes, les géodésiens, les navigateurs qui se sont, de bon gré et spontanément, rangés successivement tous à la même opinion.

Deux contrées résistent par système à ce large courant: la France et l'Irlande. Pourquoi? C'est peut-être le cas de répéter: "Tu te fâches, donc tu as tort." Pour l'Irlande on comprend bien qu'elle ne veuille rien provenant de sa redoutable et cupide dominatrice, et qu'elle ait à ce sujet l'opinion que traduit en Portugal le proverbe ancien: "D'Espagne pas de bon vent ni de bon mariage." Pour la France cependant la raison ne saurait être celle-là, mais c'est en tout cas un sentiment d'ombrage et de rancune. Ce n'est pas parce que le méridien de Greenwich a des inconvénients quelconques qu'elle ne le veut pas pour méridien primaire; ce n'est pas parce que la puissante Angleterre veut

l'imposer aux faibles (elle n'y pense même pas); c'est parce que ce n'est pas le méridien de Paris. Les savants français qui s'occupent de la question sont déjà arrivés, comme M. Cugnin, à un stade suffisamment élevé au-dessus des petites considérations de nationalité pour ne pas prétendre, qu'au lieu de Greenwich, on doive adopter Paris, car en absolu, l'un est aussi bon que l'autre; mais ils n'ont pu encore franchir le dernier degré de l'envie ou de l'orgueil national, et demandent toujours qu'au moins ce ne soit pas Greenwich! et seraient peut-être capable de rejeter toute la réforme si utile, si logique de M. Cugnin, si elle prenait pour base ce méridien exécré. Eh bien, cela n'aboutirait qu'à l'isolement, et la France vient de montrer, même dans l'ordre politique—aux ombrageuses susceptibilités s'il en fût—qu'il n'y a qu'avantages et plaisir à abandonner une tour d'ivoire d'intransigeance outrée.

Ce n'est pas une affaire de pouvoir maritime ou mondial, qui a rendu si populaire le méridien de Greenwich: c'est un avantage réel. Ce pouvoir de l'Angleterre peut bien être la cause de l'expansion de sa langue, de son commerce, de son industrie. Il est sur ces deux derniers points rudement battu en brèche par l'Allemagne et pas les Etats-Unis; comme puissance prépondérante sur les mers elle aura bientôt à compter aussi avec l'ambitieux Japon. Mais quand même l'heure de la décadence aurait sonné pour elle, quand la croix de Saint-Georges ne flotterait plus au vent que sur de rares bâtiments, perdus dans la foule des puissants du jour, qui l'auraient supplanté, même alors, les raisons d'aujourd'hui seraient les mêmes pour recommander l'adoption du méridien de Greenwich, qui a conquis ses droits, non par la force de sa propre nationalité, mais par l'opinion raisonnée des autres.

Le fait est que, par les facilités qu'ont accordées ses câbles télégraphiques, étendant le plus vaste et le plus serré des réseaux de ce genre (le seul presque jusqu'à ces derniers temps) sur le monde entier, le système des longitudes déterminées sur le globe se rattache directement à Greenwich. Or, si par exemple on voudrait en adopter un autre, il faudrait compter sur une erreur originelle provenant de l'erreur inconnue qui aurait affecté la détermination de ce méridien. Sans parler de Bombay, proposé par M. Cugnin, supposons que ce soit Paris qu'on adopte. Eh bien, ce n'est que tout récemment, et au prix de considérables efforts de longs travaux d'une perfection raffinée, qu'on a pu obtenir avec une suffisante exactitude la différence de longitude entre cette capitale et l'observatoire anglais. Cette erreur d'origine entacherait inutilement toutes les longitudes connues, si ce n'est celles directement reliées à Paris. Or si l'on voulait partir de Bombay, ou d'ailleurs, une pareille erreur, nécessairement plus considérable, rendrait fautives d'autant les longitudes adoptées, en sus de l'imperfection même de chaque détermination.

Bombay n'est pas d'ailleurs assez près de l'équateur, si l'on tenait à cette situation en latitude: ce serait plutôt Quito, dont l'Observatoire est aujourd'hui habilement dirigé par un astronome français. La circonstance d'être un port de mer fréquenté n'est nullement nécessaire, car aucun navigateur n'a le loisir d'aller régler ses montres à un port déterminé et unique: ce qu'il lui faut c'est que dans chaque port, où il arrive, l'heure du méridien initial (et par conséquent la longitude) soit connue assez exactement. Or c'est avec Greenwich que la plupart des ports sont reliés: c'est donc l'heure de ce méridien qui peut y être connue avec le moins d'erreurs.

Du moment où la réforme importante, et d'une urgence absolue, la division décimale du cercle et du jour entiers, deviendrait un fait accompli, ou au moins une probabilité prochaine, un grand pas serait fait dans le voie du progrès général de l'humanité, et de l'obéissance à cette loi naturelle du moindre effort que l'homme néglige encore si souvent. La vie ordinaire aurait fatalement à se régler sur des heures peu différentes de l'heure locale, soit par le système de M. Cugnin, soit par un nouvel agencement de fuseaux horaires; donc le choix d'un méridien initial n'aurait guère d'importance que pour les savants. Quant aux géographes et aux navigateurs, ils n'auraient qu'avantage à continuer à partir de Greenwich, comme aujourd'hui ils le font tous, sauf les Français, et s'il se manifestait à l'avenir, pour une cause inattendue, un motif de préférence indubitable envers une autre origine, la division décimale rendrait beaucoup

plus simple le changement, et d'une importance beaucoup moindre. Peut-être prendra-t-on de nouveau le méridien de Ferro qui réellement ne se trouvait pas sur l'île de ce nom, et ce rapportait conventionnellement à celui de Paris; ou celui de la Grande Pyramide, certainement le monument humain le plus approprié, historiquement et matériellement, pour servir de jalon universel et d'une durée future la plus longue probablement; ce méridien va d'ailleurs prendre une importance considérable lorsqu'une chaîne ininterrompue de triangles géodésiques rattachera le Caire au Cap et prolongera, autant que le permet la forme des continents, celle qui grâce aux travaux du grand Struve, part du cap Nord.

Ancient and Modern Ideas about the Milky Way.

[Continued from p. 342.—Conclusion.]

THE most interesting of these clusters is certainly the one which includes our solar system. M. Stratonoff designates this by the letter A. It is situated a little to the north of the median plane of the Milky Way, and extends more towards the north than to the south. Its extent scarcely exceeds a space equivalent to twice the distance from the Sun of stars of magnitude 6.5.

Three other clusters, viz. those which are nearest to us after the one just mentioned, occupy respectively the constellations Auriga, Puppis, and Scorpio. The nearest points of these are separated from us by the mean distance of stars of 6.6 magnitude; their most distant parts are at the distance of stars of the ninth magnitude.

Further, M. Stratonoff has determined the positions of five other clusters all very near to the Milky Way. We can fix the distance of the nearest point of each. Their depth goes beyond the limits of catalogues, but, according to all appearance, not beyond the capacity of existing powerful telescopes. Their delimitation will be then only a question of time and patience. It seems, considering the discontinuous appearance of the Milky Way, that these clusters are not projected in a crowd upon one another, but that they constitute rather a comparatively slender line in the form of a spiral or ring with two convolutions.

It may be added that this way of conceiving the structure of the sidereal universe is in accord with what may be learned from the best modern photographs when these are carefully studied.

In a note I have indicated where satisfactory reproductions on paper of some of these photographs have been published, but the pictures on glass that I have had the opportunity to examine have always seemed more faithful to nature and more instructive. I owe my thanks for such opportunities specially to Dr. Isaac Roberts and to Mr. W. H. Wesley, Assistant Secretary of the Royal Astronomical Society.

The existence of vast agglomerations of stars possessing a distinct individuality, seeming to form curves and streams, appears clearly on several negatives of Prof. Barnard*. Generally these great swarms are projected upon a particularly dark background,

* Monthly Notices R. A. S., March 1899.

as if they had absorbed the surrounding stars. There are some star-groups in Perseus which appear just as Prof. Barnard has pointed out when they are photographed in a large field.

When nebulae are met with in the Milky Way they generally seem in physical relation to the stars. Either they fall at the boundary between a rich region and a poor one, or else, just as the clusters do, they show a disposition to cause an empty space around them *.

In other cases the cosmic matter stops at the same limits as a group of stars, forming, as it were, an atmosphere, thus showing that they are associated †.

These stellar clouds, of which one cannot doubt the existence, become in their turn the elements of new assemblages and make up well-determined figures. Often they are arranged in the form of a ring, in the hollow interior of which float isolated clusters. There are in a manner small Galaxies within the large one ‡.

To find systems comparable in extent and complexity with the Milky Way properly so-called, it is necessary to go to the spiral nebulae. These nebulae are met with, as we have seen, at all distances from the Galactic plane, and do not seem to be in physical relation with the stars. They are not resolvable in our instruments, but those of them, such as the great nebula of Andromeda, whose light permits us to go a little further and to study their spectra, have a spectrum of a stellar nature; and it is thought that their irresolvability is only a consequence of their extreme distance. All show upon plates of long exposure a similar remarkable structure; from a central condensation two luminous streaks emerge diametrically opposite. These two streaks curve in the same direction: they branch off, they pass through numerous alternations of dimming and brightening, and stop after having described half a convolution or more. An observer placed near the centre would see therefore around him an irregular band, strengthened or doubled in a part of its course. The nebulae with relatively faint central condensation and with developed spirals present to us the appearance most like to that which the Milky Way would look to us if we could see it from outside §.

Our present means of observation do not give us reason to expect much new progress in the study of spiral nebulae. But it may be imagined that the analogy could be pushed farther if we knew better the plan of the Milky Way, and we may in this respect found great hopes upon a more extended application of the method of M. Stratonoff, an application which will become possible as soon as we are in possession of the International Photographic Chart.

P. PUISEUX.

* Roberts, "Nebula in Monoceros," *Knowledge*, 1891 Nov.; "Nebula H. V. 37 Cygni," *Knowledge*, 1898 Nov.; "Nebula N.G.C. 1499 Persei," *Knowledge*, 1899 Feb.

† Barnard, *Monthly Notices R. A. S.*, 1899 March; Dr. Max Wolf, *Knowledge*, 1891 Dec.

‡ Roberts, *N.G.C.* 2237-9; Barnard, *Monthly Notices R. A. S.*, 1899 March.

§ Roberts, *Knowledge*, 1900 June, 1898 Feb., 1897 Feb.; Wilson, "Nebula in Canes Venatica," *Lick Obs.*; "Nebula in Ursa Major."

The St. Swithun's Day Tradition.

A LETTER from a Dumfries correspondent in the October number of *Symons's Meteorological Magazine* has suggested an inquiry as to the verdict of the Greenwich records on this subject. I have examined the figures for 64 years (1841 to 1904), counting only the days when 0·005 inch was registered, and it certainly seems that the tradition does not find very strong support.

In some few years (*e. g.* 1856–1863, when the rain-gauges were only examined at 9 P.M.) there is a little doubt as to the day on which rain actually fell, but it is of no great consequence.

In no year were there more than thirty-one or less than four wet days in the forty succeeding July 15, so that the probability of absolute accuracy of the tradition vanishes at once.

In twenty of the years at least half of the forty days were wet, but in only five of these years was St. Swithun's Day wet, and it was dry in the two years showing most wet days in the forty. On the other hand, although in the two driest seasons, with only four and six wet days respectively, in the period under consideration, St. Swithun's Day was dry, yet in 1887, after a wet July 15, only nine of the forty days were wet.

From another point of view, the heaviest rainfall during the forty days, which was 9·73 inches in 1903, followed a dry St. Swithun's; and of the sixteen years in which that period produced over 4 inches of rain, only two had a wet St. Swithun's.

It would seem, therefore, that the legend does not apply to Dumfries or Greenwich. Perhaps Winchester records might tell a different tale, but it is not likely, even if they allowed for the change of style, which would alter the date by a week.

It is noticeable that there has been no rain at Greenwich on July 15 since 1896, and in the whole period there are 41 dry festivals as against 23 wet ones.

On an average, a wet St. Swithun's entails 17 wet days out of the next 40, with an average total rainfall of 3·13 inches, and a dry one brings the same number of wet days with an average total rainfall of 3·33 inches.

W. W. B.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Constellations formed by Bartsch and by Hevelius.

GENTLEMEN,—

The name of Jacob Bartsch is not well known. He was born at Lauban, in Lusatia, in the year 1599 or 1600, and died at the same place in the thirty-third year of his age, only three years after the death of his famous father-in-law, Kepler. He published at Strasburg, in 1624 *, his 'Planisphærium,' with three

* A second edition appeared at Nürnberg in 1661, twenty-eight years after the author's death.

star-maps, in which he introduced several new constellations. Three of these—Camelopardalis, Monoceros, and Musca—are still recognized. Camelopardalis was intended to gather under one name the comparatively small stars (not one of which is quite equal to the fourth magnitude) between Cassiopeia and Ursa Major. Monoceros covers the space which is crossed by the Milky Way between Canis Major and Gemini; nearly all its stars are below the fourth magnitude. Musca (or, as Bartsch calls it, Apes) is a very small group a little above the back of Aries.

Hevelius* was the originator of a larger number of constellations, all of which, except Mons Mænalus (on which one of the feet of Boötes is supposed to be resting) and Cerberus (the latter really a small part of Hercules), are now recognized. Of Triangulum Minus I wrote a few lines in your number for June (p. 239 of the present volume). Canes Venatici is situated to the south of the tail of Ursa Major, between the body of the bear and Boötes, for whose use they are supposed to be intended; it has one star of the third magnitude (Flamsteed's No. 12, called also α), which has the special name Cor Caroli. Lynx lies to the south of part of Camelopardalis, between Auriga and Ursa Major. Hevelius called it Lynx, sive Tigris, but the former designation has prevailed. He also named a constellation Vulpecula cum Ansere, which is sometimes called Vulpecula et Anser. But as that designation is so long, it is usual to ignore the goose supposed to be held in the little fox's jaws, and to call the constellation simply Vulpecula. It is situated to the south of Cygnus, between Hercules and Pegasus; the star called α , and numbered 6 in Flamsteed's list, is of about the $4\frac{1}{2}$ magnitude, and is the brightest in the constellation. The little group named by Hevelius Scutum Sobiescianum, in honour of the great victory of John Sobieski, King of Poland, over the Turks at Vienna in 1683, consists also entirely of small stars, the brightest being of only the fourth magnitude; this constellation is situated on a portion of the Milky Way to the north of Sagittarius. Finally, we have Sextans, a small constellation nearly on the celestial equator, to the south of Leo, and possessing no star of greater brightness than Flamsteed's No. 15, which is of the $4\frac{1}{2}$ magnitude, and almost due south of Regulus. Hevelius denominates it Sextans Uraniae, but the first word only is retained.

Ideler says ('Untersuchungen über den Ursprung und die Bedeutung der Sternnamen,' p. 353) that Bartsch formed seven new constellations. Of three of these I have spoken. Two others are supposed to represent rivers, viz., the Tigris and the Jordan: the former he makes to indicate a stream of stars passing from the neck of Pegasus over the Milky Way, just above the head of Cygnus, to the vacant space between Ophiuchus and Aquila. The course of the latter is at first between Ursa Major and Coma, and afterwards along nearly the whole line between Ursa Major and

* His 'Prodromus Astronomiæ' was published in 1690, three years after his death.

Leo; Bartsch suggests another name for it, viz., the Euphrates. Bartsch also inserts in one of his maps a small constellation which he calls Gallus, and places between the back of Canis Major and the hind feet of Monoceros. In his list of constellations in the extreme south he includes a very small one called Rhombus (situated between the two Nubeculæ), which (as he notes) was first called so by Habrecht of Strasburg. La Caille afterwards lengthened the name of this into Reticulus Rhomboidalis.

Yours faithfully,

W. T. LYNN.

Blackheath, 1904, Oct. 4.

The Mobility of the Earth and its first Teacher.

[Translation.]

GENTLEMEN,—

I have just read in your excellent publication Mr. Lynn's letter with the above title, in which he states that the first man who recognized the movement of the Earth was Aristarchus of Samos. Will you allow me to make some remarks?

Aristarchus of Samos was contemporary with Archimedes. The latter, in his work 'Arenarius,' which I have now before me, in fact discusses the statement of Aristarchus about the movement of the Earth around the Sun, published before the publication of the 'Arenarius.' I have also before me the work of Aristarchus on the distances of the Sun and the Moon, and there can be no doubt about his astronomical opinions. But he concerns himself here with dates and with the question of priority. We have two positive dates for Aristarchus: one, which has been given us by Ptolemy in the 'Almagest'; this is in connection with the observation of the summer solstice made by Aristarchus in the year 280 B.C.: the other date is that of the condemnation of Aristarchus (anticipating that of Galileo) made in the year 264, according to Fabricius, or 296, according to Fortin D'Urbain. Admitting that our astronomer was thirty years of age in the year 280, he would have been born about the year 310, that is 160 years after the death of his compatriot Pythagoras.

Besides, it was notorious in old times that the system of the movement of the Earth was asserted not by Pythagoras himself, but by the Pythagoreans. It is enough to read Aristotle, Cicero, Diogenes Laertius, and Plutarch to know that. Philolaus, Heraclitus, Ecphantus, Hicetas of Syracuse are there all said to have asserted it. These disciples of Pythagoras are earlier than Aristarchus by more than a century.

About this "Mobility of the Earth" it is important to distinguish the diurnal movement from the annual movement. As for the diurnal movement, it is not doubted that the Pythagoreans whom we have named had said this before Aristarchus. But the latter could well have been the first who announced the annual translation.

That the diurnal movement had been put forward by the

Pythagoreans, Aristotle has said clearly :—*τὴν δὲ γῆν ἐν τῶν ἀστρῶν αὐσαν, κύκλῳ φερομένην περὶ τὸ μέσον νύκτα τε καὶ ἡμέραν ποιεῖν*. It was, besides, to the Pythagoreans that Copernicus declared himself to belong in his work, and again the celebrated decree of the Congregation of March 15, 1616, which issued the interdict against the work of Copernicus, designated the new system in the following words:—"Falsa illa doctrina pythagorica divina scriptura omnino adversans."

It seems to me, then, that it would be highly unjust to take from the school of Pythagoras the fame that the writers of antiquity and Copernicus himself have accorded to it, making the first attempts towards the discovery of the true system of the world.

I am, Gentlemen,

The Observatory of Juvisy,
France, 1904, Oct. 3.

Yours very truly,
CAMILLE FLAMMARION.

[Mr. Lynn remarks that by "Mobility of the Earth" he intended no reference to axial rotation, but to orbital motion. The view that the Sun, not the Earth, is the centre of the system, has often been ascribed to Pythagoras.]

The Heights of Meteors.

GENTLEMEN,—

It is usually the case that the greater the velocity of meteors the more elevated are their positions above the Earth. The very swift, streaking meteors, of which the Leonids afford a typical example, attain incandescence at the highest elevations, while very slow-moving objects, like the November Andromedids, are good examples of the opposite kind, which penetrate deeply into the atmosphere and approach comparatively near to the Earth's surface. The Leonids are a little more lofty than the Perseids, the averages being:—

	Height at beginning.	Height at ending.	Number of meteors.
Leonids	84 miles.	56 miles.	25
Perseids	80 "	54 "	40

For very slow meteors the mean heights are much less, their descents being

from 65 to 38 miles ;

but there appears to be two well-defined classes involved in this value, viz., those which are directed from very low radiants and those directed from pretty high radiants, the figures being:—

	Height at beginning.	Height at ending.
Radiants very low	64 miles.	48 miles.
Radiants pretty high ..	66 "	28 "

The very swift meteors begin to burn in a visible manner when nearly 20 miles higher than the very slow meteors. The latter

with high radiants reach a distance 20 miles nearer the Earth than those with low radiants. The very slow meteors with low radiants have very long paths, and their velocity seems less than that of any other class of meteoric phenomena.

Of the Lyrids, Quadrantids, and the other periodical showers, few specimens of these have had their real paths satisfactorily computed. The Quadrantids give a mean height of 67 to 52 miles (seven meteors), while four Lyrids give 84 to 50 miles.

Yours faithfully,

W. F. DENNING.

Bristol, 1904, Sept. 5.

OBSERVATORIES.

PERTH, WEST AUSTRALIA.—Mr. W. E. Cooke has probably not a large staff of assistants, but they must be sufficiently energetic, for we see in his Report for the year 1902 that more than 5000 zone-stars were observed with the transit-circle in R.A. and N.P.D. during the year, as well as 967 clock-stars and stars for adjustments. These zone-stars are the reference stars for the plates of the Astrographic Catalogue that this Observatory is undertaking. At the end of the year 1902, 120 Catalogue plates had been taken and 68 Chart plates with triple exposure.

The standard clock of the Observatory transmits automatically an hourly electric current to regulate the railway and telegraph-office clocks in Perth. Time-guns are fired at 1 P.M. (time of the 120th meridian) at Perth and Fremantle.

But a great deal of Mr. Cooke's time and attention is directed to Meteorology. He not only supervises and receives Reports from out-stations, but he ventures to give daily forecasts; and these, he says, have been remarkably successful—in fact, during the year 93 per cent. were correct, and besides Mr. Cooke says:—“In February the first scientific attempt ever made to predict general rains throughout the interior for several days in advance was attended with complete success.”

WINDSOR, NEW SOUTH WALES.—Mr. John Tebbutt's private observatory at the Peninsula, New South Wales, has existed 40 years, and now that he has reached his seventieth year the proprietor finds that he is compelled to discontinue systematic work. His declining years will be devoted to business of a private nature with occasional attention to astronomical observation. This is the principal, if unwelcome, fact to be gathered from Mr. Tebbutt's Report of the year 1903, which he says is to be the last of the series.

Those of our readers who follow the work of observatories recorded in these pages will have noted that for many years past Mr. Tebbutt has conducted the work of his observatory in a systematic way, quite similar to that of a public institution. He has a standard clock, or rather a chronometer, Poole No. 2962,

whose error is found day by day by means of the transit instrument; air-pressure, temperatures, and rainfall are observed, so that he supplies all the elements of a public observatory, and besides he has an equatorial telescope which he uses with much effect. Comet observation has been Mr. Tebbutt's speciality. He began work at Windsor, as above implied, in 1864 (he had also observed ten years earlier than this), and in the 'Monthly Notices' of that year there will be found observations of a comet and elements of its orbit made by him. Since that time there can scarcely have been a comet visible in the southern hemisphere which he had not observed. In the present report there are notices of observations of Comets 1902 III. and 1903 III. His excellent observations of occultations of stars are well known, and many observations of double stars and occasional phenomena made by him will be found in the publications of the R.A.S. It seems a pity that this observatory should disappear simply because age is overtaking Mr. Tebbutt. Why should not the Colonial Government buy it and provide a staff?

MAURITIUS.—The work of the Royal Alfred Observatory, Mauritius, is mainly magnetic and meteorological, but some astronomy is done there, for it supplies photographs of the Sun to complete the records of the Solar Physics Committee. The Report of Mr. T. F. Claxton, the Director for the year 1903, states that 498 photographs were taken during the year on 306 days. Time determination is also made, and the time of the fall of a ball dropped at 1 P.M. by military signallers is observed. It is not stated whether this is 1 P.M. local time, but it is believed that the Universal Time System has not yet been adopted in the colony. The observatory does not seem a pleasant place to live in. Both the Director and the Chief Assistant have been ill; it seems impossible to get good computers owing to the unhealthiness of the situation; there has been a plague of white ants which destroyed the library, and the Director reports, as an unusual fact, that the water-supply has not been polluted this year.

PUBLICATIONS.

STUDIES IN ASTRONOMY*.—Of all the sciences Astronomy seems to lend itself more than any other to be the subject of popular magazine articles—not the cheap kind of article which tries to bewilder its readers by the recital of immense distances and the fancy picture of the train going to the Sun, or the more inventive kind which people the Moon or Mars—but a better kind giving interesting information not generally known, which can be read and understood easily by anyone taking up the article at random. One cannot but be struck with this fact on taking up

* By J. Ellard Gore, F.R.A.S. London: Chatto & Windus, 1904. Price 6s.

Mr. Gore's book. He has collected articles on no less than twenty-four subjects that he has contributed to the *Gentleman's Magazine*, *Knowledge*, or to this Magazine, as well as three others hitherto unpublished, into a book of 312 pages. It is difficult to make selection of the best when all are good, but it occurs to us that the articles on "Spectroscopic Binaries," "The Darkness behind the Stars," and "The Distances of the Stars" have considerable merit, as giving much information not generally known. "The Coming Comet," one of the hitherto-unpublished articles, which tells us about Halley's Comet expected to return in 1910, is useful.

NOTES.

COMET NOTES.—The Heidelberg photographic observation of Encke's Comet on Sept. 11 was confirmed by another taken there on Sept. 17. They agreed in showing that the ephemeris required a correction of $-43''$ in R.A., $-6'$ in Dec. M. Kaminsky deduces that the mean anomaly needs a correction of $-8''.06$. He applies this to form an ephemeris, which is given here reduced to Berlin midnight:—

	R.A.			N. Dec.		R.A.			Dec.
	h	m	s			h	m	s	
Nov. 1	23	17	45	24 42	Nov. 29	21	18	30	10 30 N.
5	22	58	41	23 8	Dec. 3	21	3	50	8 9
9	22	40	1	21 19	7	20	49	20	5 36
13	22	22	5	19 20	11	20	34	10	3 1
17	22	5	0	17 13	15	20	16	32	0 17 N.
21	21	48	48	15 1	19	19	56	38	2 58 S.
25	21	33	24	12 47	23	19	35	12	6 31 S.

Dr. Smart has pointed out that the comet will approach Mercury pretty closely next January. It is to be hoped that observations will be possible after the approach, as an opportunity would thus be afforded for testing the mass of Mercury.

A. C. D. C.

MINOR PLANET NOTES.—The following planets have been discovered at Heidelberg. The first two were given last month, but without details:—

Planet.	Date.	Discoverer.	Mag.
OU	1904 Sept. 11	Götz.	12.5
OV	11	Kopff.	13.3
OX	19	"	13.0
OY	Oct. 3	Götz.	11.7
OZ	9	Wolf.	13.2
PA	10	Götz.	12.1
PB	14	"	11.0
PC	14	"	12.0

Planet 460 (1900 FN) has been named Scania. A. C. D. C.

OBITUARY. — G. H. WITH. The death is announced of Mr. George Henry With, of Hereford, whose name is most generally known as that of a maker of specula for reflecting telescopes, but his early career and training was far from being of a mechanical kind. He was born in 1827, and after his school education in London, spent two years at St. John's College, Battersea, training for the profession of schoolmaster, and was one of the first Certificated Masters of England. After a first appointment he was appointed Master of the Blue Coat School, Hereford, in 1851, and here he spent twenty-five years; but beyond his school-work he devoted much time to science, and, in conjunction with his friends the late Rev. Cooper-Key and the Rev. J. W. Webb, brought to perfection the art of making specula, and there are many examples of his work extant.

AN HONOUR FOR MR. DENNING.—Our readers will be pleased to hear that our frequent contributor, Mr. W. F. Denning, has been granted a pension of £150 a year from the Civil List in recognition of his services to astronomy. Mr. Denning was born in 1848, and began his astronomical work in 1865. His name is chiefly associated with the observation of meteors; and his discovery of the moving radiant of the Perseids of August and of "stationary radiants" (that is, of meteor-streams which endured for several days with a radiant-point remaining fixed among the stars) is classic. Besides this, as our readers well know, he has devoted much time to the observation of the surface-markings of Jupiter and Saturn, and has written many papers on these subjects. He has discovered five comets and some nebulae, and in 1898 February was awarded the Gold Medal of the Royal Astronomical Society for his meteoric observations and cometary discoveries. He received the Valz Prize from the Academy of Sciences, Paris, in 1895. We offer our sincere congratulation to Mr. Denning on this official recognition of his merits, and wish him good health to enable him to enjoy his new fortune.

THE ST. LOUIS EXPOSITION.—It has been already stated in these pages that a Congress of Astronomers was held at St. Louis during the recent exhibition. The writer of our Oxford Note-Book will no doubt say something about this when the account of his travels reaches St. Louis in order of time. Meanwhile we may say that besides Prof. Turner, part of whose address appears in this number, Prof. E. C. Pickering spoke on the photometric work of the Harvard Observatory, a report of which will appear in the 'Popular Science Monthly.' Prof. W. W. Campbell's address on Spectroscopy will appear in the 'Astrophysical Journal,' and no doubt the speeches delivered by Professors Kapteyn, Boss, and Backlund will be printed in other publications.

THE following have been elected as Officers and Council of the British Astronomical Association for the ensuing Session:—

President: A. C. D. Crommelin. *Vice-Presidents*: E. W. Maunder, S. A. Saunder, G. M. Seabroke, C. T. Whitmell. *Treasurer*: W. H. Maw. *Secretaries*: J. G. Petrie, J. A. Hardcastle. *Librarian*: F. W. Levander. *Other Members of the Council*: C. L. Brook, A. Cottam, Tyson Crawford, Henry Ellis, W. Heath, Mrs. E. W. Maunder, G. J. Newbegin, Mrs. Isaac Roberts, D. Smart, C. Thwaites.

WITH reference to a note in our last number, in which the name of M. Flammarion was mentioned, that gentleman writes to us that though he has now no official connection with the National Observatory, Paris, his astronomical career began when he was only 16 years of age in the Bureau des Calculs of the Observatory, and four years later he was transferred to the Bureau des Longitudes, where he helped to compute the *Connaissance des Temps*. Also, at the invitation of Le Verrier, he made his well-known series of observations of double stars with the great equatorial of the Paris Observatory in 1876-1877, and that in this work Madame Flammarion acted as assistant to her husband, which may be the circumstance to which the writer we quoted referred; but M. Flammarion points out that the statement made in the Canadian publication would have been true if the name of the observatory of Juvisy had been written for that of Paris.

We hasten to assure M. Flammarion that our correction was written solely in the interest of truth, and with no *arrière pensée* of disrespect.

THE terms of the will of the late Dr. Isaac Roberts have lately been announced. It appears that after providing for Mrs. Roberts the residue of his estate, which will probably amount to about £35,000, has been left to be divided equally between the Universities of Liverpool, North Wales (Bangor), and South Wales (Cardiff), for the foundation of Scholarships.

READERS need scarcely be reminded that the time to look for Leonids this year is the morning of Tuesday, November 15. Remembering last year's experience, it is not unlikely there may be a good display.

THE next Meeting of the Royal Astronomical Society will be on Friday, November 11, at 5 P.M.; of the British Astronomical Association on Wednesday, November 30.

FROM AN OXFORD NOTE-BOOK.

[THE journey in the United States, referred to in these Notes for the last few months, was completed in October, but there is fortunately still "copy" enough from the notes of travel to furnish one or two more numbers. Perhaps I may be allowed, however, to interpolate a brief note here on the visual observation of the satellite Phoebe, which has attracted some attention. It was

mentioned (see p. 355) that on arriving at Harvard we heard that the existence of Phœbe had been established photographically from 35 plates. An attempt was made one evening while I was there by Prof. E. C. Pickering and Mr. Wendell to see it at Harvard with the 18-inch, but without success; indeed it was clear, from the difficulty of seeing stars shown more clearly than Phœbe on the photographs, that the 18-inch was not powerful enough to show the satellite. On proceeding to the Yerkes Observatory I was fortunate enough to interest Prof. Barnard in the search. The only available ephemeris (that giving places for the three nights July 14, July 24, and August 3) had run out by the time a favourable opportunity offered, viz. on August 8; but by graphical extrapolation a rough place was found. Curiously enough, there was an object exactly at the place thus indicated, but obviously too bright for Phœbe. A diagram was made of the faint stars near the place, and sent to Prof. E. C. Pickering, but as no indications of motion were detected (possibly the time was too short), we were not hopeful that the satellite had been caught. But on comparing the diagram with photographs at Harvard, Prof. Pickering immediately saw that there was an extra object in the diagram. [What a wonderful weapon that store of photographs is, and how plainly this is shown at every turn!] Writing to me under date August 12 he says:—"Your object *d* appears to have been Phœbe. . . . There is no star in this position shown on photographs taken with the Bruce telescope on which Phœbe is visible."

But I did not get this letter until long afterwards, and had meanwhile written the words on p. 390 recording our search as unsuccessful. It was delightful to learn that we were mistaken, and that we had really seen the satellite. Prof. Barnard, not being a rolling stone, gathered the moss of this good news much earlier, and was able to obtain ample confirmation of it by finding the satellite again and *measuring* it. My own part in the observation was a very minor one, and is only worthy of note from the following circumstance. When I had arrived at the Lick Observatory somewhat later, Prof. Hussey kindly consented, on September 12, to make a search with the 36-inch. We had a good ephemeris from Prof. W. H. Pickering this time, and were able to recognize that the satellite was, without much doubt, one of three close faint stars, but which of them we could not tell without some further test. Prof. Hussey tried to measure them in order to detect motion, but found them *too difficult*, and, having been present on both occasions, I should say that all three stars were decidedly more difficult objects as seen in the Lick telescope than that which we saw in the 40-inch and which turned out to be Phœbe, and which, moreover, Prof. Barnard has measured. So far as it goes, this experience is interesting as a comparison of the two instruments, but of course it does not go very far. The difference may have been entirely due to the quality of the night, and it is to be remarked that the next night (September 13) was cloudy at Mount Hamilton and prevented Prof. Hussey from identifying the

satellite by a re-examination of the region (on September 14 Saturn had moved too near the place). But it seems possible that Phœbe may be of just the right brightness or faintness to afford such a convenient comparison of the relative powers of the instruments, and I shall look forward with interest to its subsequent history in this respect.

One other word. It seems that my cautious statement, that "there is a remarkable feature about the orbit [of Phœbe] which, however, I must not mention, as the discoverer wishes to make the first announcement himself in a number of the Harvard 'Annals' just passed for press" (see p. 355), was not sufficient to hide the secret from Mr. Crommelin, who promptly divined it, and sent his solution to the *Astronomische Nachrichten*. He tells me that my words actually put him on the scent! Well, I cannot be sorry for the indiscretion, for Prof. W. H. Pickering, whom I have seen since the betraying words were in print, made no complaint of them, and his priority is in any case sufficiently well established, whereas Mr. Crommelin has had the satisfaction of making an independent discovery from very scanty material, which does him immense credit, and I beg to offer him my hearty congratulations, in which I feel sure Prof. W. H. Pickering will cordially join.]

Somewhere in Arizona, 1904, Aug. 28.

It would be affectation not to regard the railway journey we are at present taking as somewhat remarkable. A brief account of it may interest those who have not travelled in this country, though they must be cautioned against setting out in the hope of repeating our experiences, for we no doubt had exceptional luck. Still, the complacency with which our fellow-passengers have accepted most of the incidents indicates that something of the kind is within the reach of other tourists. As a preliminary, it may be stated that we hoped to make a three days' journey from Chicago to Flagstaff (stopping there on the chance of finding Mr. Percival Lowell at home); from thence an expedition to the Grand Cañon of Arizona, and then completing the journey to California, to see what Prof. Hale was doing on Mount Wilson, and to visit the Lick Observatory. We thought that this was a fairly straightforward business. But we are now better educated.

THE first part of the programme was carried out with tolerable fidelity, the journey to Flagstaff being varied by nothing more noteworthy than half of the train running off the line. Novices like ourselves could scarcely avoid some little alarm when we saw the track torn up for 50 yards by the heavy baggage-cars which formed the front of the train, and which had run some distance through the sleepers before the powerful brake had pulled us up short and sudden. And of course there was necessarily some delay. The passenger-cars were backed away and taken round the scene of damage on the parallel line, and though we felt grateful to be personally safe, we could not help wondering, as we passed the

stranded cars, how long it would be before we saw our baggage again. But we did not then know the resources of the country; these heavy cars were dug out and sent after us so speedily that they caught us up during the night, and we ultimately arrived at Flagstaff only about four hours late—more punctually, indeed, than the trains of several days previously.

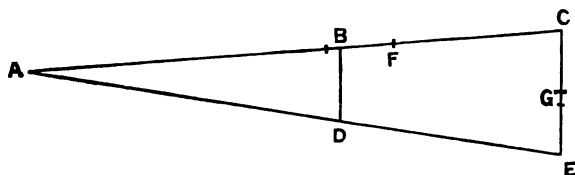
MR. LOWELL was unfortunately not at home; and though we were treated in the most cordial and hospitable manner at his observatory, I shall not venture to describe it in his absence. Even Shakespeare thought it better not to write the play of Hamlet without including a part for the chief character. But we saw the platform in the woods from which Mr. Lowell had once made a Fourth of July oration; and we were glad to hear his praises sung by our driver, who commended the observatory also (Mr. Lowell is generous to visitors of all kinds), and gave it an honourable place among the attractions of Flagstaff, though he obviously thought we should spend our evening better in seeing the steam saw-mills at work by the electric light, when they looked much prettier than in the daytime.

THE journey to the Grand Cañon, though nominally of only three hours' duration, was richer in incident than our previous one, for the train caught fire at least four separate times. By a certain irony it was the refrigerator cars which were partially burnt, sparks from the engine setting them alight near the roof. Buckets were filled at the engine and carried by willing hands to the flames, and those who could not get buckets stood on the forest and watched. Each time the fire was got under the train started with no particular warning, and in climbing hastily in on one occasion a man dropped his hat. It will give some notion of the time-scale to remark that at the next stop he ran back for it, and triumphantly returned with it before we had moved on again. But soon the fire-extinguishers became so skilled that it was no longer necessary to stop for them to work, and we steamed quietly along, with our busy corps of firemen pattering to and fro on the roof of the train with their buckets, until the refrigerator cars were damp enough to refuse further ignition.

THE Grand Cañon is fortunately indescribable, so that no attempt to say anything of it need be made. Some weeks ago one who had seen it said it was the finest thing in the world, and when the query "You think so?" was put almost mechanically, he rejoined emphatically "I know so!" Now we have seen it we "know so" too. We meant to spend one day there, and changed our plans at sight so as to make the one day into four. But there is a man of real intelligence who spends his whole vacations there; he has a tent close on the "rim," so that when he awakes in the morning he gazes at once on the whole glorious picture. There is a mine in the depths of the Cañon, whence its magnificence is seen from below, and so devoted are these miners that they will stay six or

eight months at a time in their little colony without ever wishing to visit civilization. One of them was going to the St. Louis Exhibition, and I felt much tempted to apply for his job, only that I know so little of mining.

It was on leaving the Grand Cañon that our real education in American railway-travelling began. To explain our evolutions I



must ask the Editors to allow me a simple diagram like a long thin letter A with the feet joined. We were at the point F, near the cross-bar, and wished to reach A (Los Angeles), which, in the ordinary course, we should have done in 18 hours, the distance being about 500 miles. But there were rumours of trouble between A and B in the shape of "wash-outs," for it had been raining pretty steadily and was still doing so; in fact its persistence drew from one of our fellow-passengers the remark that "if this goes on we shall have to do what they do down in Arkansaw when it rains." "What is that?" said I, falling into the trap. "Why! they just let it rain." And so did we. Our progress to the point B, however, though slow, was uneventful. The surprise came when we arrived at B to find three trains pulled up there to await events, and we learnt that three more had gone on a little further down the line. The "wash-out" between A and B was serious; the damage had been repaired with great labour, only to be repeated by another storm, and it was feared that several days would be required to restore the line for traffic. Unfortunately an alternative route, BDA, was also blocked by a wash-out in BD, so that after waiting as a forlorn hope for better news we were not much surprised to receive the order at night that the whole seven trains were to return to C and reach A by the route CEA. We were the last of the procession, for some of the trains had already been waiting several days, and our sleep was delayed by the sound of our departing companions in trouble. But we woke to find ourselves well on the road to C (Albuquerque), and completed the journey thither before night. But then came a long wait for three other cars of which we were to take charge, and it was not till the third day that we started for E. After proceeding some 70 miles, we received orders to return to C.

We began to get interested in the game. There were some undesirable features of course; there were two babies in our car for whom it was difficult to get milk at times, as supplies of all kinds were running short; and some passengers in the tourist cars, who had not much money with them, were not well off. But on

the whole the delay was taken with wonderful good temper ; our fellow-passengers declared that they had always wanted to see that part of the line thoroughly ; and we began to know station C really well, for we had another delay there. Presently we were off again to B, and on arrival we learnt that the route BDA was now open, and we proceeded accordingly. We have now passed D without mishap, and it seems probable that we shall at last complete our journey before dawn to-morrow. It is tolerably warm (102° F. in the car and higher outside), for we are passing through a desert below sea-level ; for the last few days we have only had one regular meal a day, supplemented by such scraps as we could carry on board ; and we were made anxious this morning by news of another wash-out ahead of us. But the prospect of getting to our journey's end makes us cheerful in spite of these little drawbacks, and one of the company has written the following poem in honour of the voyage : I hope the terminology will not be obscure to English readers :—

THE BASEBALL PLAYER'S DREAM OF OUR WANDERINGS.

1. Last night I dreamt a curious dream,—
D'y'e care to hear the tale ?
We played a game, as it would seem,
Of baseball, but *by rail*.
2. In length the diamond seemed to be
A thousand miles—terrific !
One half belonged to Santa Fé,
One half to South Pacific.
3. We started with a three-base hit,
And "home" in sight we reckoned ;
But then the umpire took a fit
And called us back to "second."
4. And even this was not the worst ;
He said—and gently smiled—
"The right way 'home' is round by 'first,'"
Which nearly drove us wild.
5. We started off for "first," but when
His fool-mistake he found,
That madman called us back again
To run the right way round.
6. So once again we got to "third"—
Again we had rude shocks ;
"The way 'home' now," he sent us word,
"Is through the 'pitcher's box.'"
7. Setting our teeth, we turned about
And headed for the "pitcher."
(Our pockets meantime giving out,
For travellers don't grow richer !)
8. The "pitcher" sent us "home" at last
(God bless him for that shot !);
And though our pace was not so fast,
We certainly were *hot*.
9. A wondrous ride we rode that week,
That merry company ;
And if another game they seek,
May I be there to see !

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EDITED BY

T. LEWIS, F.R.A.S.,

H. P. HOLLIS, B.A., F.R.A.S.

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VOL. XXVII.

DECEMBER, 1904.

No. 351.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1904 November 11.

Prof. H. H. TURNER, M.A., F.R.S., *President*, in the Chair.

Secretaries: F. W. DYSON, M.A., F.R.S., and
E. T. WHITTAKER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

The President. It will be within the knowledge of the Fellows that since our last Meeting death has been unusually heavy in laying his hand upon our fellow-Members. We lost in the summer, first, Captain Noble, a very well-known Fellow and for many years a very regular attendant at these Meetings; then we lost Dr. Isaac Roberts, whose work—and more than his work, the publication of it, which he defrayed so generously at his own expense—is so well known in the astronomical world. To-day some of us have been present at the funeral of Mr. Frank McClean, whose generous benefactions to astronomy, which extended to the foundation of Studentships in Cambridge University, and also to the provision of a large telescope at the Cape of Good Hope Observatory, have been fully appreciated by us all. He also did important work which, like Dr. Isaac Roberts, he published at his own expense. The Council this afternoon passed three resolutions more or less in the same words, and it was thought that the Fellows would like to have the opportunity of associating themselves with the Council in these. I will read the one concerning the loss of Mr. McClean:—

That the Council have learned with deep regret of the death of Mr. McClean, who for 27 years was a greatly esteemed and distinguished Fellow of the Royal Astronomical Society, who, moreover, was for 10 years a Member of the Council and received the Gold Medal of the Society; and the Council desire to

put on record their profound sorrow which Astronomy has sustained by his death, and beg to express their condolence with Mrs. McClean and the bereaved family, and that a copy of this resolution be sent to Mrs. McClean.

The other resolutions are similar in character, mentioning, in the case of Dr. Isaac Roberts, that he was for 22 years a Fellow, and for 6 years a Member of the Council, and that he held the office of Vice-President; further, that he had received the Gold Medal of the Society. In the case of Captain Noble, we mention that he was for 49 years a Fellow of the Society, and that for 27 years he was a Member of the Council. I will now put it to the Fellows, who will vote in silence, by a show of hands, whether they will associate themselves with these resolutions.

The Fellows present unanimously agreed to the passing of the resolutions.

Mr. Whittaker. 168 presents have been received since the last Meeting. Among them are the following books:—R. Buchanan, 'The Mathematical Theory of Eclipses' (presented by the Author); Groningen Astronomical Laboratory, Publications, Nos. 12, 13 (presented by the Laboratory); 'Annals of Harvard Observatory,' vol. xlv. pt. 2, vol. liii. nos. 1, 3, vol. lvi. no. 1 (presented by the Observatory); 'Catalog der astronomischen Gesellschaft [Leiden and Wien-Ottakring Zones] (presented by the Society); Milan Observatory, 'Albategnius, Opus Astronomicum, Latine Versum' (presented by the Observatory). We have also received 60 Charts of the Astrographic Chart, presented by the Royal Observatory, Greenwich, and 59 Charts presented by the French Minister of Public Instruction; 28 Cartes Autographiées (presented by the Toulouse Observatory); 7 great enlargements from photographs of the Moon by MM. Lœwy & Puiseux (presented by the French Minister of Public Instruction); series of 24 transparencies from the late Dr. Roberts's photographs of Nebulæ (presented by Mrs. Isaac Roberts); Wire Micrometer by Dallmeyer (presented by Mrs. Irving Noble); Old Ring Dial (presented by Dr. Little).

The President. I would call special attention to the gift of Mrs. Isaac Roberts, and I believe she will allow me to say that she was only carrying out the wishes of her deceased husband, for it was known to her that Dr. Roberts intended these transparencies for presentation to the Society. The number of papers offered at this Meeting is so large that I think it would be more convenient if we did not put these slides in the lantern at the present time, though at a future Meeting we may have an opportunity of admiring them. Besides, as they are of a large size they are not particularly well adapted for showing on this screen. I would ask the Fellows to return their thanks to the donors of these valuable presents.

The President announced the name of a gentleman who was no longer a Fellow of the Society, as he had neglected to pay his subscription, in accordance with Bye-Law 28.

The President. We have what I take to be an unusually

interesting list of papers this evening. It would be invidious to draw distinctions, but we have one paper from Mr. Maunder on which I hope there may be some discussion. I propose to take the papers in the following order:—First, Mr. Cowell's paper on the long-period terms in the Moon's longitude; next, Mr. Saunder's on the measurement of positions of craters on the Moon; and then Mr. Maunder's paper. There is also a paper by Mr. Dyson and Mr. Thackeray, and some minor papers will be read if time will allow.

Mr. P. H. Cowell then read a paper on "A Discussion of the Long-Period Terms in the Moon's Longitude," and explained it by means of a lantern-slide which showed the errors of the Moon's longitude before and after two new empirical terms had been added to the tables. Mr. Cowell explained also, with the help of the slide, his method of analysis. The errors were developed in powers of the time as far as t^6 . Coefficients of high powers of t could only arise from terms of moderate periods; terms of very long period gave zero coefficients from t^6 upwards. For the Moon's errors the coefficients from t^6 upwards were not zero. One coefficient, for instance, was 70 with a probable error of 4. This establishes the existence of terms of moderate period. Mr. Cowell also considered that he had proved that the period of Newcomb's empirical term (273 years) was 30 per cent. too small.

The President. The question of new terms in the lunar theory carries us back a good many years, I am afraid. I would ask Fellows if they have anything to say upon this paper.

Mr. Crommelin. Is there any explanation in theory of these terms? I gather from a paper in the Supplementary Number of the 'Monthly Notices' that the term of 66 years' period has no sensible coefficient. I should like to know if the term of 42 years or the term of 340 years have any explanation.

Mr. Cowell. There is no theoretical explanation at present of either term.

The President. I think that the point with regard to watching the high powers of the time is entirely new. It is new entirely to me, but I do not know whether Mr. Cowell can trace any previous use of it.

Mr. Cowell. The method is entirely original.

The President. I think we are all ready to express our admiration of Mr. Cowell's work without being at the present moment in a position to criticise it. I will now call upon Mr. Saunder to give some account of his laborious work in measuring the position of objects on the Moon.

Mr. S. A. Saunder then read a paper on "The Determination of Selenographic Positions, and the Measurement of Lunar Photographs. Third paper: Results of the Measurement of Four Paris Negatives." In two previous papers which I have had the honour of presenting to the Society I have called attention to the

inadequacy, both in number and in accuracy, of the measured positions on which our standard maps of the Moon are based, and I have described experiments which seemed to show how the deficiency might be made good by the measurement of photographs.

Since my first paper appeared, Dr. Franz has published two lists containing in all 308 carefully measured positions, and I am to-day able to present a catalogue of 1434 positions obtained from 2302 measures made on four Paris negatives for which I am indebted to the kindness of M. Lœwy. Two of these negatives were taken under morning and two under evening illumination.

The mean probable error of a point measured on three plates, as determined from the residuals, is $\pm 0''.07$, for one measured on four plates it is $\pm 0''.06$, and for one measured both at the telescope and on the photographs it is $\pm 0''.04$. The errors shown by these residuals are partly errors of observation and partly due to the assumption made in the course of the reductions that the surface is spherical. A search has been made for systematic error, and, as the most probable cause of such error seemed to be the direction of illumination, the mean residuals were taken for all points measured once under each illumination, or twice under each illumination. The means for the co-ordinates parallel to the Moon's equator for the plates under morning illumination were $-0''.05$ and $0''.00$, for those under evening illumination they were $+0''.05$ and $0''.00$. At first sight it looks as if there were a small error due to this cause, but it was noted that the first two plates had negative and the two latter positive libration in longitude. It was found that the errors due to the assumption of a spherical surface would have the same signs as those actually found, and at present there is very little evidence for any systematic error due to illumination.

I give in my paper the details of some differences between my measures and those of Dr. Franz which are hard to explain; the only suggestion I can make is that there is a systematic difference of about $0''.24$ between us in estimating the centre of Mösting A which does not extend to our estimates of the centres of other craters; but the symmetry of Mösting A, which is about $7''$ or $8''$ in diameter, makes this very hard to accept. In all other respects the comparison seems very satisfactory.

When a point has been measured on several photographs taken under different librations there should be an apparent shift of its position due to its altitude above or below the mean sphere to which its position is referred in the reduction of the measures, and if the measures are of sufficient accuracy the absolute altitude of the point may be determined. I give the theory in the paper, and attempt to apply it to the 38 points measured on all four of my photographs and all lying near the central meridian. The results are not very good; the average probable error of a determination is about half a mile, the absolute values ranging from 3 miles below to $1\frac{1}{2}$ miles above my mean sphere. This work,

however, requires very accurate measurement. Of the 304 residuals on which the determination of these 38 altitudes depends, only four amount to 0".5; the average value is 0".13. Assuming the mean surface of the Moon to be a spheroid with its axis of figure directed towards the Earth, I find from my results that the elongation of the Moon towards the Earth, *i. e.* the value of $\frac{a-b}{a}$, where a, b are the semi-axes of the spheroid, is $+0.0052 \pm 0.0027$. The best previous direct determination with which I am acquainted is that given by Franz in 'Die Figur des Mondes,' which is $+0.0114 \pm 0.00390$. Dr. Franz, in his determination of the physical libration, also found that the dynamical elongation, determined from the moments of inertia, was $+0.006$. This does not necessarily coincide with the geometrical elongation, but it is interesting to note how nearly it does agree with my value.

I have, in conclusion, to express my thanks to the Government Grant Committee for grants by the aid of which much of this work has been done.

The measures have all been made by Mr. J. A. Hardcastle; the accuracy of the results is due to the great care and skill with which he has overcome the difficulties inseparable from the measurement of unsymmetrically illuminated lunar formations. A considerable part of the reductions have been made with great care by Mr. B. G. C. Gray, of the Oxford University Observatory. Mr. J. C. W. Herschel has given me valuable help in applying the final checks to the catalogue as well as in other ways, and is now engaged in plotting the measured points on an index map, part of which I am able to show upon the screen. The preparation of the catalogue has involved an amount of work which would have been far beyond my unaided powers, but our President has allowed me to feel that I have had all the resources of the Oxford University Observatory behind me, and has supplemented these with personal advice and encouragement the extent of which it would be difficult to adequately acknowledge.

Mr. Hardcastle. Of course it is only with regard to the actual measuring at the micrometer that I can speak at first hand, and two points are of interest. First, with reference to those craters where it appeared easy to locate the crest—in these cases one knew that the dark area on the negative [Blackboard] was larger than it should be, and the question arose as to how much. Luckily we had means of forming an estimate, for I had already measured several points on two positives—not included in this paper—which were under opposite illumination. The discordances of the reduced positions showed that I was about seven of our units wrong in each case. Accordingly, whenever contrast was strongly marked, I deliberately cut into the apparent crest by about four of our units; with less contrast by about two or one units. The second point refers to craters near the limb which were exceedingly

elongated, and it was merely by guesswork that one selected a crest. The effects of this uncertainty showed themselves in the probable errors of the deduced altitudes. I would take this opportunity of expressing my sense of the advantage it has been to me to do this first piece of astronomical work under so kind and competent a leader as Mr. Saunder.

Mr. Hinks. I have only to express my admiration of the measuring work in this matter. Knowing how difficult it is to get results of high accuracy of star-images, I must say it is extraordinary that Mr. Hardcastle and Mr. Herschel have been able to get such excellent results, and I shall look forward with interest to reading the full details of the paper. It seems to me a most interesting and extraordinary thing that one should be able, by pure judgment and pure cleverness, to get over all the difficulties which the uneven shape of the craters and the unevenness of illumination must necessarily have lying in wait for you.

Mr. Dyson. Mr. Saunder mentioned an error possibly arising from personality in the measures of Mösting A. I should be glad if Mr. Saunder will show this crater on the screen, and perhaps say a little more about the difficulty in determining the exact position, and what gave rise to the difference in estimation by the different measurers.

Mr. Saunder. I have nothing to say as to why it is so; it is a puzzle to me. The crater is a most beautiful round circle. It is much better at the telescope than on the photograph. It comes out so well that it makes my explanation all the more difficult to receive myself.

Mr. Crommelin. About the physical librations, I should like to ask Mr. Saunder if he can throw any light upon the controversy between Prof. Franz and Prof. Hayn (who reduces the libration to one quarter of the value found by the former). I do not know whether Mr. Saunder's result throws any light on the question.

Mr. Saunder. I do not think my examination will throw any light on the physical librations of the Moon. You want to measure a large number of photographs ranging over two or three years. Prof. Franz's value of the physical libration was obtained from a series of heliometer measures made by Schlüter. Hayn's measures also were made at the telescope. If anyone would set to work as Prof. Pritchard did on modern photographs to the extent of 100 or 200, spread over two or three years, he might get a better result. My own work consisted in measuring a small number of photographs.

The President. It is not often that in the science of measurement the probable error is reduced from 10 to 1. I think we may all congratulate ourselves that it is a Fellow of our Society who has been able to take that important step in lunar topography. I will now call upon Mr. Maunder to read his paper.

Mr. Maunder, in describing a paper on "Magnetic Disturbances 1882 to 1903, as recorded at the Royal Observatory, Greenwich,

and their Association with Sun-spots," said:—This paper is in continuation of two which I communicated to the Society at the January Meeting this year. In the first of these I took the 19 greatest magnetic storms in the last 30 years, and ascertained what was the state of the solar surface at the time of each. The result of that paper, in my opinion, was to show that there was a relation between the great magnetic storms and the great sun-spots. For the present paper I took all the magnetic disturbances of which the traces are reproduced in the plates given in the Greenwich Magnetic Results. I went through these and made a catalogue of them, about 310 in number. From that catalogue I struck out a few that were distinctly of smaller importance than the rest, so as to get a lower limit of magnetic disturbance, which should be roughly the same for different years. Then for eight months I worried at that catalogue, first in one way and then in another. I took out the groups of spots, about 800 in all, which were on the Sun at the time of those disturbances and made comparisons between them. The result was very much what I had got out 12 years before, very much what Father Sidgreaves had admirably summarized in his paper of 4 years ago. There was a general rough correspondence between sun-spots and disturbances, a good number of cases in which the agreement between the two seemed decidedly striking; and also of cases where there were so many spots on the Sun at the time of a disturbance, that it was impossible to associate it with one rather than with another. But there were also a fair number of remarkable discrepancies. Sometimes there were great spots on the Sun and no magnetic disturbances, and sometimes no spots and great magnetic disturbances. Then I tried the spots of each hemisphere, North and South separately; I tried the faculæ, then the prominences, first altogether, then North and South separately, then polar prominences alone. The general result was always the same—in each case a rough general correspondence,—but the details did not show any satisfactory agreement. Then quite recently—perhaps not quite a couple of months ago,—looking over my catalogue of disturbances, I was struck with four coming near the end of 1886, which followed each other at approximately equal intervals of time. Four more in 1887 followed each other in like manner, the mean interval in both series being 27 days 8 hours. But 27 days 8 hours is about the apparent rotation-period of the Sun for this time of the year, and that suggested to me another inquiry. Of the nineteen great storms which I had examined in my earlier paper, sixteen synchronized with the appearance on the Sun of a very large group of spots, but three did not. These three synchronized with the return of sun-spots, which had been very large in the preceding rotation, but were now small. It struck me that it would be well to take the different disturbances from my Catalogue and supply the longitude of the centre of the Sun's disc for each disturbance. Going through the Catalogue I found there were some 36 sequences in

which the disturbance in one rotation was followed by a disturbance in the next rotation with the same solar longitude. Of those 36 sequences, eight extended to a third rotation, four to a fourth, and one to a sixth. In a good many other cases we had a sequence in which a rotation was missed out. The same longitude recurred, not in the next rotation, but in the next but one. Taking these two classes together, nearly one-half the disturbances catalogued were included in one or other of these sequences. When longer intervals were taken, more than three-fourths of the total disturbances—276—fell into one or other of these sequences; in short, the solar rotation-period was found to be stamped upon the entire catalogue of these disturbances from the beginning to the end. It seems to me there are a great number of points which follow from the recognition of that fact. I think that supposing we had known the solar rotation-period, as we might have done from spectroscopic observations, if we had never seen a spot or a prominence on the Sun we could still have said that our magnetic disturbances were due to some action on the Sun. That is the first conclusion. The next is that the solar action comes from certain distinct and definitive areas of the Sun's surface, not from that surface as a whole. Thirdly, that action takes place along a very definite line of direction. If there were only a few of these correspondences we might suppose each was due to some explosion taking place on the Sun. We know the great magnetic storms begin suddenly, simultaneously all over the world, and we might explain each as being due to a sudden violent action on the Sun. But when we find a disturbance repeated again and again—as in one case no fewer than six successive times and practically at the same interval of time,—I do not think that explanation will hold at all. Whatever conception we make of the character of the solar action, whatever be the nature of the solar influence, it must be something which has struck the Earth like a stream or current—something not radiating equally in all directions like light and heat, but having a definite direction. Another point which comes out is that different rotation-periods are given by different sets of disturbances. The shortest gives a daily angular motion only 5' greater than found by Carrington for the solar equator. The longest, so far as I have detected, corresponds with the rotation-period for latitude 30 degrees; the great majority corresponding with the mean rotation-period, viz. that for latitude 14 degrees. We therefore have the solar rotation-period brought out not only in the mean rotation, but also for the extreme rotation-periods of the zones where sun-spots are mostly found.

If we take it as established that we have really a solar action of this nature, then the next conclusion is that there must be many spots the influence of which can never reach our Earth, because the action, whatever its nature, being along certain definite lines, and our Earth occupying only a small angular space, it must often miss our Earth. We have no right to say of any particular spot that

it ought to stir up our magnets. But we can go further and get some sort of insight as to the way magnetic disturbances may take place when there is no such thing as a spot on the Sun. In the very valuable paper which Father Sidgreaves gave us four years ago, he took two instances as showing that the cause of magnetic disturbances did not lie within or near the Sun. These were two which fell in November 1889, when there were no spots on the Sun. But they were the last two of a series of six, following each other at intervals of 27 days. I do not think we can doubt they were practically parts of one and the same action going on continuously for those five months. If that is so, and we are to regard this as one single action, we must go back to the first. There were only three great spots in the whole of that year, 1889. The largest was in its second rotation, when the magnetic disturbance began. The third spot in size formed in front of this first one, about the time the disturbance commenced. Both spots came round in the next rotation, and so did the disturbance. But after the spots had disappeared the disturbance returned, and returned again for four rotations further still. In this case it is clear that the magnetic activity of the Sun could be prolonged after its spot-activity had ceased to be visible to us. One or two side issues are touched upon in my paper: one is a comparison of the great disturbances which I gave in my last paper with sequences of disturbance, which seems to suggest that this influence of the Sun need not be truly radial. Nearly all the spots in question were in high latitudes, and it seems improbable, if it were truly radial in character, that their influence could have reached us. If we take the average length of the disturbance as an indication of the breadth of those disturbing streams, then as 30 hours represents $16^{\circ}5'$ of solar longitude, 20° would represent the mean diameter of these disturbing streams, supposing them circular in section. Another relation that suggested itself to me in looking through the list was that of a number of cases in which the disturbance in one rotation was answered by the disturbance in the same longitude 13 or 14 rotations afterwards, that is to say in the interval of a solar year. There were 28 or 29 of these if we confine ourselves to a range of 10 degrees in longitude. We should expect, in the ordinary way, that about half that number would be purely accidental coincidences, but one case I think is so striking that it cannot be a chance one. Only three disturbances are shown in the catalogue for 1900, and in each case there was a disturbance at the same longitude 13 rotations earlier. That all the three disturbances of a single year should correspond with disturbances exactly 13 rotations earlier, I think is too much to expect as a mere accident. Then we have instances of disturbances occurring at irregular intervals over long periods of time. Let me give two instances of such long-continued irregular sequences. There were 16 disturbances in the course of the period 1895 February 9 to 1896 March 26. Of these 16, 9 clustered round one longitude and 3 round another,

leaving only 4 out of the 16 sporadic. Here, again, we have an association which is too striking to be accidental. Yet another case. The great sun-spot of February 1892 was the largest seen during 30 years. Shortly after it passed the centre of the disc there was a very great magnetic storm, and as it returned there was another very great magnetic storm. The longitude of the centre of the Sun was 238 and 233 degrees respectively at the times when those two storms commenced. In 1893 that longitude was continually active throughout the entire year, and indeed is certainly more active than any other in the whole catalogue of 276 disturbances. Is it possible that this particular area of the Sun has been intermittently active for several years? this very area which gave rise to the largest spot we have had on the Sun for over 30 years. May I refer you again to the photograph Mrs. Maunder took of the corona of 1898 in India, for I think it illustrates the way in which we may visualize the method in which the solar action takes place? This photograph showed us four long streamers or rod-like rays proceeding from the corona. The photographs obtained by Mrs. Maunder in Mauritius in 1901 show the building-up of the roots of these rod-like rays by particles driven away from the Sun in successive curves. Prof. Arrhenius has suggested that under certain conditions minute droplets are formed by condensation in the Sun's atmosphere, and driven away by the pressure of radiation. Streams of such droplets, charged with negative electricity, might account for our magnetic storms. But however this may be, I would suggest that in the results I have here brought forward, we are on the track of solving what Lord Kelvin spoke of twelve years ago as "the 50-years' outstanding difficulty."

The President. I think Mr. Maunder has justified what I said at the commencement about this being a most important paper, and I do not think that the Fellows present can wish the discussion of such an important matter to be curtailed.

Father Cortie. I only had notice of this very important paper just before I came to the Meeting this afternoon, otherwise I might have been able to say more than I can say at present. I must congratulate Mr. Maunder on the paper itself, which is a most valuable contribution to an important subject, though I am not prepared to follow him on all the points dealt with. Reserving, then, any detailed criticism until the appearance of the paper in print, I will only bring forward one or two general objections. With regard to all hypotheses which consider magnetic storms as caused by direct radiation from the Sun, I cannot see that anyone so far has met the theoretical difficulty proposed by Lord Kelvin in 1892. Taking a magnetic storm in 1884 as an example, the difficulty is this, that "in the eight hours of a not very severe storm, as much work must have been done by the Sun in sending magnetic waves out in all directions through space as he actually does in four months of his regular heat and light."

If I understand Mr. Maunder rightly, he supposes the magnetic regions on the Sun to be the causes of the magnetic storms. There is no solar physicist who does not admit the close connection between solar activity and terrestrial magnetic storms, but the point at issue is precisely whether the connection is one of efficient or instrumental cause and effect. I cannot follow Mr. Maunder when he postulates that the fact of there being sometimes relatively big spots unaccompanied by magnetic storms may be explained by supposing that such rays terminate in space before they reach the Earth. For instance, one would have expected that such a spot as that of 1901 May should have been accompanied by a magnetic storm. Here was a spot that was relatively large and active, accounting for 74 per cent. of the total spotted area of the year. It was born, too, at the time of the solar eclipse, and its position was marked by a fine prominence and a disturbed region in the solar corona. It passed across the visible disc during two rotations. In spite of Herr Nippoldt's recent article in the *Astrophysical Journal* (Vol. xx. No. 3, Oct. 1904), I am prepared to maintain that there was no unusual disturbance of the magnets during its passage across the Sun. The biggest swing of the declination magnet during the year occurred on May 10, and consisted of a single movement of 40' of arc. On that date the spot was either not born, or was on the side of the Sun turned away from the Earth. It seems to me that when there is an unusual disturbance of this sort on the Sun during a period of calm, there should be some answering magnetic storm, if the connection is one of cause and effect. But perhaps when I read Mr. Maunder's paper in print I may be converted. I am most anxious to be converted, but at present I am not *.

Sir Robert Ball. I am sure we are under a great debt of gratitude to Mr. Maunder for this interesting paper. I think that that most striking demonstration of his on the screen is one we shall never forget.

Mr. W. W. Bryant. Yesterday afternoon, knowing of Mr. Maunder's paper, I had the curiosity to turn up my note-book and I noticed that the last four magnetic storms were on July 6, Aug. 3, Sept. 25, and Oct. 21. The first and last were of exactly the same character—a sharp fall in declination and an increase of the horizontal force, followed a little later by a change in declination only. Carrying those dates back by periods of

* [It was in my mind at the Meeting to refer to a statement made by Professor Schuster at the Belfast Meeting of the British Association in 1902, on a subject cognate to the subject under discussion. Being unable to give an accurate quotation I passed it over. The statement was as follows:—"As the result of my calculations I can definitely state that the magnetic declination at Greenwich shows no period between 25.5 and 27.5 days having an amplitude as great as 6". The influence of solar rotation on magnetic variation may therefore be considered to be definitely disproved." Would not a periodicity of about 27 days due to magnetic storms, apparently caused by magnetically active regions on the Sun, have an effect on the variation of the amplitude of magnetic declination?—A. L. C.]

27·8 days we arrive at Oct. 31 last year, which everyone will remember was marked by a decidedly large magnetic disturbance.

Mr. W. Ellis. I would congratulate Mr. Maunder on his continued attention to the question of relation between solar phenomena and variations in terrestrial magnetism, which from his close acquaintance with the former subject he has so good an opportunity of following up, and I shall be glad of the opportunity of studying his paper in print. My comparison of magnetic disturbances at widely different places * having shown that at the commencement of great disturbance all parts of the Earth appear to be simultaneously affected, as it would seem from without, a state of general magnetic calm being everywhere instantaneously converted into one of magnetic activity, involving the whole Earth, the circumstance is one that seems, so far as I can at present gather from Mr. Maunder's paper, to favour his views. But however caused, the action as regards magnetic disturbance is subject to quiet periods of considerable length, as, for instance, about the year 1879 and at other times. In our latitude there is also greater magnetic activity in spring and autumn than in summer and winter, midwinter being especially quiet, a phase of action deserving attention.

Mr. Newall. Several speakers' remarks make one wonder whether our method of procedure is the best one. Hardly anyone has spoken without expressing the difficulty he feels in speaking without having read the paper, and at the same time expressing the pleasure with which he is looking forward to a chance of reading it. Would it be possible to have the discussion on papers of this kind at a later Meeting than that at which it is presented, when there is only one person in the room who knows all about it, and he the writer of the paper? However, I think everyone present must feel in a position to congratulate Mr. Maunder upon the vigour with which he has carried on the work. I would like to express my appreciation of the work that is being done in attacking these streamers from the Sun at both ends.

Mr. Crommelin. With regard to Father Cortie's remarks, I think that Lord Kelvin made the assumption that these streams of force from the Sun were distributed equally in all directions, so that the Earth only received a small portion of the whole discharge. It may be worth while to throw out the rather wild suggestion that possibly these discharges may be somewhat of the nature of electric sparks between the Sun and the neighbouring planets, so that the latter play some part in influencing their direction. Mr. Maunder showed on the screen a picture of the great coronal streamer of 1898; it was remarked long ago that it pointed almost exactly towards Venus.

If there is anything in this suggestion, the diameter of the stream of force would be of the same order as the diameters of the planets, and consequently the total amount of energy involved

* Proc. Roy. Soc. vol. lii. page 191.

would be only an extremely minute fraction of that deduced by Lord Kelvin; this would diminish the difficulty of supposing the Sun to be the efficient cause of magnetic storms. It would also tend to explain the great number of these disturbances which we get, because if the streams are narrow we should not expect the Earth would go through so many of them unless it had some power of attracting them to itself.

Mr. Newall. The difficulty is when we get a hose-like stream of corpuscles directed upon a certain part of a planet; when a certain point is reached the planet must be covered with something which is repelling the arrival of future corpuscles. It is necessary for the theory to be effective to have "sinks" as well as "sources."

The President. Mr. Newall's suggestion about our procedure is a very interesting one. It is not, however, possible to discuss it now, but I hope he will pursue his idea, for I think that our discussions may be improved by the adoption of some such system. It is very pleasing to be in the Chair when such important papers are presented as these three: the first showing a definite step in theoretical astronomy, the second a very great advance in the practical work of the measurement work of the Moon, and then this new idea from Mr. Maunder.

Mr. Maunder. Of course, the paper stands or falls upon the fullness with which the solar rotation period is shown by the disturbances, and that, of course, I can hardly ask the Meeting to take on my word, but when they see the Table I hope they will conceive that I have taken the right view of the position.

Mr. Dyson read a paper, by *Mr. Thackeray* and himself, "On the Determination of the Precession and the Direction of the Solar Motion from a Comparison of Groombridge's with Modern Greenwich Observations." The Greenwich Second Ten-Year Catalogue, which was made from observations extending from 1887-1896, contained most of the Stars in Groombridge's well-known Circumpolar Catalogue. A comparison between Groombridge's and modern Observations would clearly provide a large amount of new material for the discussion of the important questions of precession and solar motion. Much attention has been given to the proper motions obtained by Auwers from Bradley's observations, by Newcomb, Kapteyn, L. Struve, and others, and it seemed likely that a determination of proper motions from Groombridge's observations would be of value. In view of the large systematic and accidental errors in Groombridge, it was necessary to re-reduce the observations.

A picture of Mr. Groombridge and of his transit-circle was thrown on the screen, and it was explained that Mr. Groombridge was an amateur astronomer, who after retiring from business set up a transit-circle at Blackheath, and made 25,000 observations with it.

Tables were thrown on the screen showing the distribution of

the stars according to magnitude, proper motion, and type of spectrum. The large proper motions and the uniformity of distribution of different proper motions of Type II. stars were pointed out.

Tables were also shown of the mean proper motions in right ascension of stars of different magnitudes for each three hours of right ascension, showing an agreement in the means for the 24^h of bright and faint stars. The value of the precession constant was derived partly from these mean results and partly from the consideration that stars of large and small proper motions should give the same position for the right ascension of the apex of the solar motion. This was obtained both with Newcomb's and with Struve's precession, and it was concluded that the former value needed a small correction towards the latter.

The position of the apex of the solar motion was determined for (i) stars of different magnitudes, (ii) stars of large and small proper motions, and (iii) for stars of Type II. The different results were nearer than might have been expected, and the result from Groombridge for the R.A. of the apex was about 274° , and for the Dec. $+34^\circ$ or $+37^\circ$, according to the weights given to different classes of stars.

Mr. Thackeray. I should like to emphasize this fact, that in analyzing these proper motions the chief difficulty lies in dealing with the question of possible systematic errors and with systematic differences of the proper motions themselves, and to meet this difficulty we divided them up into arbitrary groups of centennial proper motion, $0''-5''$, $5''-10''$, $10''-20''$, and over $20''$. This method serves a double purpose—it differentiates between the values of the apex from different classes of stars, and at the same time it forms a somewhat delicate gauge for showing up systematic error, because the values of x and y in the equations depend on quantities of either descending or ascending magnitude. The table thus formed of the values of the R.A. of the apex depending on proper motions in R.A. and N.P.D. demonstrates that the value of the precession constant can only lie between very small quantities, unless the systematic errors of this determination of proper motions happen to counterbalance one another in the two elements of R.A. and N.P.D.

The President. I think I have seen some of these results about the motion of the solar system before, at the time when I was enquiring whether there was an apparent rotation of bright stars with respect to faint. At that time there seemed to be evidence from the measurements of photographs that there was something of the kind, and two papers were published on the subject which were more or less in contradiction. I am able now to mention the possible cause of that discrepancy. It may be it was due to defects in the position of the object-glass at the time that the photographs were taken. I think I had better put that on record until the

matter has been more carefully examined. It may be that the discrepancy is caused by an instrumental error.

Mr. Hinks. I would ask whether Mr. Dyson can say what is the relative weight of the determination of the right ascension and declination of the apex of the Sun's motion. I take it the apex lies upon the edge of the spherical cap covered by Groombridge's observations.

Mr. Dyson. The two co-ordinates are equally well determined from the declinations; but the right ascension alone is determined from the right-ascension observations. The weights in the combined equations are as three to one, but as the declinations are more free from systematic error than the right ascensions the result for the declinations is relatively somewhat better than this.

Mr. Newall. Has Mr. Dyson compared the position of the apex with that which Mr. Campbell, of the Lick Observatory, some years ago gave? Does Mr. Campbell's value come nearer to your second type than to the general mean?

Mr. Dyson. No, Mr. Campbell's is 20° and we get 37° .

The President. Mr. Campbell had only 40 stars.

Mr. Newall. No, 280; 40 regions, I think.

Dr. Rambaut. I would desire to congratulate Mr. Dyson and Mr. Thackeray on the completion of this important piece of work. In a matter of this sort it is a pity that all the material available is not used. I may remind the Society of a vast amount of information bearing upon proper motions which I, and everybody in the room, would like to see utilized for the purpose. I hope to bring this subject up before the Society from time to time until some means is found for using the information.

The following papers were announced and partly read:—

A. C. D. Crommelin. "Ephemeris for Physical Observations of the Moon, 1905."

Col. E. E. Markwick. "Note on the Variation of ϵ Aurigæ."

A. A. Rambaut. "On a very Sensitive Method of Determining the Irregularities of a Pivot: on the Pivot Errors of the Radcliffe Transit-Circle, and their effect on the Right Ascensions of the Radcliffe Catalogue for 1890."

S. A. Saunder. "The Determination of Selenographic Positions, and the Measurement of Lunar Photographs. Third Paper: Results of the Measurement of four Paris Negatives."

P. H. Cowell. "A Discussion of the Long Period Terms in the Moon's Longitude."

F. W. Dyson and W. G. Thackeray. "A Determination of the Apex of the Solar Motion, and of the Constant of Precession, from a Comparison of Groombridge's Catalogue (1810) with Modern Greenwich Observations."

E. W. Maunder. "Magnetic Disturbances 1882 to 1903, as Recorded at the Royal Observatory, Greenwich, and their Association with Sun-spots."

W. Shackleton. "Telescopic Observation of a Meteor-trail."

H. H. Turner. "Note on a Possible Source of Error in Measures of Star-Places due to Defective Centering of the Object-glass."

The following gentlemen were elected Fellows of the Society :—

Thomas Andrew Common, 63 Eaton Rise, Ealing, W.

Major C. F. Close, C.M.G., R.E., Brompton Barracks, Chatham.

The following Candidates were proposed for election as Fellows of the Society :—

Alexander John Samuel Adams, Superintendnt and Technical Officer, Post-Office Telegraphs, London Central, E.C., (proposed by William Ellis).

Capt. Arthur ffolliott Garrett, R.E., Craigbeg, Kingussie, Scotland (proposed by H. H. Turner).

P. Groves Showell, L.C.C. School for Marine Engineering, Poplar, E. (proposed by Sir William Christie).

George Bruce Halstead, M.A., Ph.D., Professor of Mathematics and Astronomy, Kenyon College, Gambier, Ohio, U.S.A. (proposed by Prof. C. S. Howe).

William T. Litton, Head Master, 'Shaftesbury' Training Ship (proposed by Thomas Lewis).

Alfred Noël Neate, Civil Engineer, 34 Prescott Street, New Brighton, Cheshire (proposed by R. C. Johnson).

Alexander Durie Russell, B.Sc., Mathematical Master, High School, Falkirk, Scotland (proposed by P. S. Hardie).

John James Steward, F.R.Met.Soc., Optician, 457 West Strand, W.C. (proposed by Wm. Shackleton).

Lewis H. Tamplin, F.R.Met.Soc., Indo-China Steam Navigation Co., Wuhu, China (proposed by C. H. Brewitt Taylor).

David Wylie, Teacher of Mathematics, 9 East Road, Lancaster (proposed by Joshua Jukes).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

President: S. A. SAUNDER, M.A., in the Chair.

Secretaries: A. C. D. CROMMELIN, B.A.
J. G. PETRIE.

THE fifteenth Annual General Meeting of the British Astronomical Association was held at Sion College, Victoria Embankment, on Wednesday, October 26th.

The President said he was sure all present were pleased to see Mr. Petrie back again, and to find that he was not much the worse for his accident.

Mr. Petrie read the Minutes of the previous Annual Meeting, which were confirmed.

Mr. Crommelin read the list of presents received, and the thanks of the Association were accorded to the respective donors.

The President drew special attention to the instruments bequeathed to the Association by the late Capt. Noble.

The names of nine Candidates for Membership were read and passed for suspension, and the election by the Council of four new Members was unanimously confirmed.

The report of the Scrutineer of the ballot for Officers and Members of Council for the Session 1904-5 was then read and the nominations made by the Council for Editor and Directors of Observing Sections were unanimously confirmed.

The Report of the Council on the work of the previous Session together with the financial statement were presented and adopted, and votes of thanks to the retiring Members of Council and to the Auditors and Scrutineers of the ballot were passed.

The President, before proceeding to deliver his Annual Address, referred in feeling terms to the loss that the Association had experienced in the deaths of Capt. Noble, its first President, and Dr. Roberts. He said that Capt. Noble had cordially welcomed and encouraged the proposal for the formation of the British Astronomical Association, he was a Member of the Provisional Committee, and had been from the first, often at great personal inconvenience, a constant attendant at the Meetings of the Council. The Association had had no more zealous and devoted Member. Within eight days of Capt. Noble's death there passed away another of their Members, Dr. Isaac Roberts, whose achievements in astronomical photography had done so much to give them a truer conception of the grandeur of the Universe in the midst of which they dwelt. Dr. Roberts had been nine times elected to serve on the Council, six times as a Vice-President, and had been approached in the hope that he would allow himself to be nominated for the Presidency, but he had declined on the ground of the state of his health. The suddenness of his death showed that this was no unreal excuse.

With regard to the progress of the Association, there was again a diminution in numbers. This was to be regretted from a financial point of view, but it did not necessarily mean that they were in any way failing to fulfil the purpose of their Founders. The real test was not in the number of persons they could induce to pay a subscription, but the number who joined the Sections to do useful astronomical work, work which, he would remind them, did not necessarily require expensive instruments or great intellectual powers. Their thanks were due to the Directors of Sections who spent so much time and trouble in arranging the work of the

Sections and upon whom so much of their real success depended. There was also another officer whose duties were no less important and even more onerous. It needed no words of his to convince them that a Journal, so well edited as theirs was, could not be kept at its present standard without a very considerable expenditure of time and care on the part of the Editor, and he was sure that every Member of the Association would join in thanking Mr. Levander for what he so ungrudgingly did for them.

The President then proceeded to give an address on the History of Lunar Photography, profusely illustrated by lantern-slides. Commencing with the discovery of the Daguerreotype process in 1839, by which, in March 1840, Dr. J. W. Draper succeeded, with an exposure of 20 minutes, in obtaining a series of Daguerreotypes of the Moon about an inch in diameter which showed the principal formations, he traced the history of the various inventions and improvements down to the latest productions of the Paris, Lick, and Yerkes Observatories. He then detailed the various theories that had been advanced to account for the peculiar features of the Moon, the ring mountains, the extensive maria, the precipitous mountain-masses, so different from anything observed on the Earth. In conclusion he discussed the possibility of there being a slight atmosphere on the Moon and small physical changes suggesting volcanic action.

Dr. Downing said the pleasant and easy task had been assigned to him of moving a vote of thanks to the President for the Address he had just given. At the commencement of the Address Mr. Saunder had made feeling allusion to the loss sustained by the death of Capt. Noble. He had had the pleasure of more than 25 years' uninterrupted friendship with Capt. Noble and he was glad of the opportunity of endorsing every word the President had said as to Capt. Noble's achievements and characteristics. But if they were fortunate in their first President the Association was also to be congratulated upon its latest President. The address to which they had just listened was sufficient to warrant his saying so. The subject was one of which Mr. Saunder was an acknowledged master. He had great pleasure in proposing a hearty vote of thanks to Mr. Saunder, both for his excellent Address and for his conduct in the Chair during the last two years.

Mr. E. Walter Maunder seconded the Motion, which was carried unanimously.

The President in returning thanks said he should like to draw attention to the fact that his successor in the Chair, Mr. Crommelin, was the third professional astronomer who had allowed himself to be nominated as President of the Association. He thought that those of them who were amateurs hardly realized all that they owed to professional astronomers for the amount of assistance which they had rendered them. He believed that the inception of

the Association was entirely due to one of those professional astronomers, and all had seen how much work they had done for the Association.

Dr. Downing read a paper (illustrated by lantern-slides) upon the "Relative Drift of Stars in the Hyades."

Mr. Crommelin pointed out that *Dr. Downing's* paper showed that it was not safe to assume that faint stars near bright ones were more distant than the latter, and that great care was necessary in using them as standards for proper motion.

Mr. W. T. Lynn read a paper on "The 9th Satellite of Saturn," and *Mr. Crommelin* gave a *résumé* of one by himself on the same subject.

The President thanked the writers of the papers. Other papers were postponed, and the Meeting stood adjourned.

ROYAL METEOROLOGICAL SOCIETY.

THE opening Meeting of the Session was held on Wednesday evening, November 16, at the Institution of Civil Engineers, Great George Street, Westminster, *Capt. D. Wilson Barker, F.R.S.E.*, President, in the Chair.

Lieut. Charles Royds, R.N., of the 'Discovery,' gave an interesting address on "Meteorological Observing in the Antarctic," and showed a number of slides referring to the recent Antarctic Expedition.

Mr. F. J. Brodie, F.R.Met.Soc., read a paper on the "Decrease of Fog in London during recent Years." He had discussed the number of days of fog reported at Brixton, the London station of the Meteorological Office, for the 33 years 1871-1903, and found that the mean annual number of fog-days was 55, of which 45 occurred in the winter half of the year, and only 10 in the summer half. December is the foggiest month with 9.5, the next being November with 8.5, January with 8.2, and October with 7.8. The clearest months are July with 0.4, June with 0.6, and May with 0.8. The greatest number of fog-days were 86 in 1886, and 83 in 1887; and the least, 13 in 1900, and 26 in 1903. Dividing the 33 years into 3 periods of 11 years each, the author showed that the mean for 1871-1881 was 55, for 1882-1892 it was 69, while for 1893-1903 it was only 41; there being thus a very marked decrease in the number of days with fog during the last 11 years.

A paper on the "Hurricane in Fiji, January 21-22, 1904," by *Mr. R. L. Holmes, F.R.Met.Soc.*, was, in the absence of the author, read by the *Secretary*. This storm caused a great deal of destruction.

*Some Reflections suggested by the Application of Photography
to Astronomical Research.*

[Continued from p. 399.]

MAY I now turn to one or two of the problems with which this new development of our work has brought us face to face? They are numerous and serious, and it is impossible to consider many of them, perhaps even the most important of them. One of the most pressing is the problem of rendering generally accessible the vast accumulations of material for study that have been suddenly thrust upon our attention. How are our photographs to be stored, preserved, and published? Even now troubles have gathered, and time will only multiply them. It is many years since Prof. Pickering drew attention to the difficulties in storing the photographic plates taken at the Harvard Observatory; when many thousands of photographs have been accumulated, not only the space they occupy but the actual weight of glass is an embarrassment. And there seems to be no doubt concerning the duty of accumulation. May I confess an early and mistaken view which I formulated on this matter? I reasoned thus:—The proper moment for making use of a photograph taken last night is to-day. It is useless to defer the examination until to-morrow, for there will then be new photographs claiming attention. Hence it is unscientific to take more photographs than can be dealt with immediately. This seemed to be a plausible argument and to show a way out of the difficulty, for if a photograph had once been adequately examined, it need not be stored so carefully, and there would not in any case be many to store. But Prof. Pickering has demonstrated many times over that the view is untenable. By taking photographs almost recklessly and without any hope of dealing with even a fraction of them, he has created the possibility of tracing the history of celestial events *backwards*. When new objects are discovered he can go to his shelves and tell us how long they were visible previous to discovery; and this information is so valuable that we must certainly arrange our future plans with reference to it. It is quite certain that we must be prepared to deal with enormous accumulations of plates, to store them in proper order, and to catalogue them; and if it has already been found difficult to do this for the collection of a single observatory during twenty years, what can we look for in the centuries to come?

Possibly the second difficulty, that of preservation, may be an antidote to the first. It is by no means certain that our photographs will last long; and if not, there will be a natural limit to the time during which they need be kept. Sir William Crookes has, however, reminded us that by toning them, by substituting sturdy gold for the perishable silver, we may prolong their life indefinitely, though this will, of course, sensibly increase the cost of each plate. As yet I have not heard of any toning process being

systematically adopted. Our course is, however, comparatively clear in this direction; it would seem imperative that a selection of the earliest photographs at any rate should be carefully toned, so that they may be available for comparison in years as far distant as possible. Although this is a matter of detail, it seems to me to compare in importance with almost any practical question which may claim the attention of astronomers; and if some decision of the kind were the only outcome of this gathering, I think we might be well content with the result.

The question of publication is chiefly one of funds, and is only worthy of special remark because these particular funds are so often forgotten in planning enterprises. I need not labour the point, for the experience of any astronomer will supply him with plenty of instances. The difficulties of publication have much in common with those of storage; they will increase year by year, and even when the money for printing has been found, the storage of publications received from other observatories will itself become an embarrassment. There is, however, one way in which some of the stress may be relieved, namely by efficient cataloguing. If we have before us a list of all the photographs existing in the world, and know that we can send for a copy of any one of them which may be required, it is no longer necessary to have copies of all. This applies, of course, to other publications as well; and though we may take some time to grow out of the sentimental desire for a complete library, and though the existence of a few such complete institutions may always be desirable, I venture to think that many observatories will ultimately be driven to the plan of acquiring only what is certainly and immediately useful, depending on temporary loans from central institutions for other material.

But there is a class of problems differing totally in character from these practical questions of storage and preservation of plates. A period of suddenly increased activity such as we have been passing through in astronomy is not without important effects on astronomers themselves. The human element in our scientific work is sometimes overlooked and generally accorded only a subordinate importance; but coming as I do from an old university devoted to the Humanities, I may be perhaps forgiven for calling attention to a few human considerations. In the first place, I have felt some anxiety lately for that very important body of astronomers who are sometimes called amateurs, though the name is open to criticism—those whose opportunities for work are restricted to a more or less limited leisure. It is a body which is somewhat sensitive to the feeling that astronomical work has gone beyond them, that in the presence of large instruments and of the special knowledge acquired by those using them, their own efforts and their own humbler instruments are no longer of any value. If I am right in supposing that this feeling has been called into existence lately by the rapid advances made in photography, it is certainly not for the first time. At previous epochs this diffidence

has found expression and has, I am glad to say, been met by careful contradiction; but it is necessary to repeat the expostulation again and again, for the anxiety is apt to crop up with every new development of astronomical activity.

The *early* days of photography were better ones than usual for the amateur; indeed the introduction of the photographic method is largely due to the work of such men as Rutherford and Draper in America, De la Rue and Common in England. But now that we have passed beyond the stage when each new plate taken was a revelation; now that we are tolerably familiar with, at any rate, the main types of possible photographs which can be taken with modest apparatus; more especially now that we have begun to discuss in elaborate detail the measurement of star-positions or of stellar spectra, the old shyness is beginning to crop up again. But it is of the utmost importance that this shyness should be zealously overcome. Perhaps, after all, it is not sufficient to assert that there is still good work for amateurs to do, nor even to mention a few instances of such work urgently required; perhaps it should be made easier for them to follow what is being done. Especially do we want more and better *books*, written by the best men in each subject. The original memoir, though it may be the proper form of publication for the workers themselves, does not satisfy all requirements. There is much to be done in the way of extension and collation before the work can be presented in a form attractive to those who would gladly keep in touch with it if the process could be made a little easier. Huxley was constantly urging scientific men that it was not sufficient to attain results; they must also express them in an intelligible and attractive form. Of course it is not easy for the same man to do both. There are few who could have determined, like Schiaparelli, that the period of rotation of the planet Mercury was 88 days instead of 1; but there are fewer still who, after making the discovery, could have given the beautiful lecture which he gave before the King of Italy, developing fully in attractive detail the consequences of the discovery; and yet it is probably true that many more could make, at any rate, an attempt in this direction, if adequate opportunity and inducement were provided. Could not a part of the sums available for the endowment of research be devoted to the endowment of text-books? It is of course an inducement to write such a book that it is a good thing well done; but in the case of a scientific worker this is scarcely sufficient, because the same could be said of his continuing his particular work. If we ask him to pause, and render the treasures he has collected accessible to others, there must be some additional inducement. Publishers are not able to offer pecuniary encouragement, because books of the type I have in mind would not appeal to a very large public. But why should they not be subsidized? I do not think it need be a very costly business, if the money were placed in the hands of a central body to issue invitations for books to be written. An invitation would be in itself a com-

pliment ; and the actual pecuniary value of the inducement would shrink in importance, just as the actual amount of gold in a medal awarded by one of our leading scientific societies is not very seriously regarded. It may be objected that to ask the best men to write text-books is to set them to inferior work, and so to delay true scientific progress ; but are we sure that the real march of science is being delayed ? There are pauses in a journey which merely waste time ; but there are others without which the whole journey may be delayed or prevented, as when a man should neglect to rest and feed the horse which carries him.

But the development of photography has brought with it much more than a recurrence of diffidence in some amateurs ; it has foreshadowed a serious rearrangement of astronomical work generally—a new division of labour and a new system of cooperation. To quote one notable instance: a very small number of observatories could take enough photographs to keep the whole world busy examining or measuring them, and we are already face to face with the question whether this is a desirable arrangement. Let me give a concrete example of this modern situation. In the winter 1900-1 the small planet Eros offered a specially favourable opportunity for determining the solar parallax, and some thousands of photographs were taken at a number of observatories for the purpose. It is not yet very clear how a definitive result will be obtained from the mass of material accumulated, most of which is being dealt with in a very leisurely manner : but a small portion of it has been discussed by Mr. A. R. Hinks, of Cambridge, and one of the many important results obtained by him in a recently-published paper (*Mon. Not. R. A. S.*, June 1904) is this : that the plates taken at the Lick Observatory are susceptible of such accurate measurement, and so numerous, that a determination of the solar parallax from them alone would have a weight nearly equal to that from the whole mass of material. If the Lick plates can be measured and reduced, it will not much matter if all the others are destroyed. Whence we may deduce two conclusions : firstly, that it is eminently desirable that these beautiful pictures should be measured and reduced as soon as possible ; secondly, that we must consider future plans of campaign very carefully if we are to avoid waste of work and discouragement of workers. It is tolerably easy to reach the first precise conclusion ; I wish it were easier to arrive at something more definite in regard to the second. It seems clear that we may expect some readjustment of the relations between the better-equipped observatories and those less fortunate, but it is not at all clear what direction that readjustment should take. One possibility is indicated by the instance before us : the discussion of the Lick photographs was not conducted at the Lick Observatory, but at Cambridge : the price paid for the fine climate of Mount Hamilton is the accumulation of work beyond the powers of the staff to deal with, and the new division of labour *may* be, for the observatories with fine

climates and equipment to take the photographs, and astronomers elsewhere to measure and discuss them. Prof. Kapteyn has set us a noble and well-known example in this direction, and in view of the pressing need for a study of many photographs already taken, it is to be hoped that his example will be followed, especially in cases similar to his own, where no observatory is in existence. If in such cases the investigator will set up a measuring machine instead of a telescope, he will deserve the gratitude of the astronomical world.

But the case is not so clear when a telescope is already in existence. Mr. Hinks had a fine telescope at Cambridge, and it required some self-denial on his part to give up observing for a time in order to discuss the Lick photographs and others. If the accumulations already made, and others certain to be made in the future, are to be dealt with, this kind of self-denial must certainly be exercised, but it does not seem quite clear that it should always fall to the lot of those with a modest equipment. Considerations of strict economy might suggest this view, but there is a human side to the argument which is not unimportant. The danger that the minor observatories should feel their work unnecessary is even graver than the similar possibility in the case of amateurs already mentioned, and calls for prompt attention from astronomers generally if it is to be averted. It is the more serious because of another set of considerations of a quite different kind, viz., the funds available for research show a rather alarming tendency to accumulate in the hands of a few large observatories, leaving many astronomers who could do useful work without the means of doing it. A conspicuous example is afforded by the present state of the work for the Astrographic Chart initiated in Paris seventeen years ago. On the one hand, a few of the large observatories have easily acquired funds not only for taking and measuring the plates and printing the results, but for publishing an expensive set of charts which will be of very little use to anyone; on the other hand, some of their colleagues have found the utmost difficulty in getting funds for even taking the plates; others have got so far but cannot proceed to measure them; and very few indeed have yet funds for printing. If there had been a true spirit of cooperation for the general good in this enterprise, surely some of the funds being squandered on the comparatively useless charts would have been devoted to the proper completion of the only part of the scheme which has a chance of fulfilment. I do not mean to imply that this would have been an easy matter to arrange, but it is noteworthy that no attempt in this direction has been made, and that as a consequence a promising scheme is doomed to failure in one important particular. For though the survey of the whole sky to the 11th magnitude may some day be completed, it will be sadly lacking in homogeneity. Some sections are finished before others are begun, so that in the vital matter of epoch we shall have a scrappy and straggling series instead of a compact whole.

Cooperation in scientific work, the necessity of which is being borne in upon us from all sides, is nevertheless beset with difficulties, and no doubt we shall only reach success through a series of failures, but we shall reach it the more rapidly if we note carefully the weaknesses of successive attempts. In the particular scheme of the Astrographic Chart, I think an error which should be avoided in future was made by those who have access to the chief sources of astronomical endowment. They have made the enterprise doubly difficult for their colleagues: firstly, by setting a standard of work which was unattainable with limited resources; and secondly by depleting the reserves which might have gone to assist the weaker observatories.

It is easier to draw attention to these modern tendencies than to suggest a remedy for them. It may, perhaps, be questioned whether a remedy is either possible or necessary; it may be urged that it is both inevitable and desirable that astronomical observation should gravitate more and more to those well-equipped observatories where it can be best conducted, and that new resources will obtain the greatest results when added to a working capital which is already large. From the purely economical point of view of getting results most rapidly, these conclusions may be true. But if we look at the human side of the question I hope we shall dissent from them; if we think first of astronomers rather than of the accumulation of astronomical facts, I hope we shall admit that something must be done to check the excessive specialization and the inequalities of opportunity towards which there is a danger of our drifting. We cannot afford the division of astronomers into two types: one isolated in a well-equipped observatory in a fine but rather inaccessible climate, spending his whole time in observing or taking photographs; another in the midst of civilization, enjoying all the advantages of intercourse with other scientific men, but with no telescope worth using, and dependent for his material on the observations made by others. Some division of labour in this way is doubtless advantageous, but we must beware lest the division become too sharply pronounced. Will it be possible to prevent its undue growth by some alternation of duties? Can the hermit observer and the university professor take turn and turn about to the common benefit? The proposal is perhaps a little revolutionary, and has the obvious disadvantages of inconvenience and expense at the epochs of change; but I do not think it should be set aside on these grounds.

I must admit, however, that I am not ready with a panacea. It has been chiefly my object to draw attention to some modern tendencies in astronomical work, hoping that the remedies may be evolved from a general consideration of them. Such questions of the relationship of the worker to his work are even harder to solve than those we meet with in the work itself. But there is at least this excuse for noticing them on an occasion like the present, that

they are, to some extent, common to all departments of knowledge, and our difficulties may come to the notice of others who have had occasion to consider them in other connections and may be able to help us. Or, again, we may take the more flattering view that the human problems of Astronomy to-day may be those of some other science to-morrow; for Astronomy is one of the oldest of the sciences, and has already passed through many stages through which others must pass. In any case we must deal with these problems in the sight of all men; and of all the consequences entailed by our lately-acquired opportunities, none are more interesting and none can be more important to us than those affecting the astronomer himself.

Frank McClean, LL.D., F.R.S.

FRANK MCCLEAN, whose loss we are all mourning, was one who, seeing work to his hand, did it; who, finding openings for work, gave opportunities for having it done; who, realizing a movement far more clearly than many a leader, refused to take the place of a leader, and, saying that he could best help the movement forward from the ranks, so helped it. What the influence of such a man can be, the years show and will show, for it is a living influence.

He was the only son, in a family of six children, of J. R. McClean, F.R.S., the distinguished engineer, whose influence and judgment may be gauged not only by his having sat upon many Royal Commissions at home, but also by the fact that his advice was sought by the Emperor Napoleon about improvements in the city of Paris, and that he was one of the English engineers at whose emphatic recommendations the making of the Suez Canal was pressed forward at a time when the practical difficulties of completing it were such that the Viceroy was almost ready to abandon the plans of Lesseps.

He was born in 1837, and when he was twelve years old began public-school life at Westminster; three years later, following in his father's footsteps, he went to the University of Glasgow, where he came under the influence of Prof. W. Thomson, now Lord Kelvin; and after carrying off many prizes there, he entered Trinity College, Cambridge, in 1855, went out as a Wrangler in the Mathematical Tripos, 1859, and was a Bachelor Scholar of Trinity for the next three years.

Then began his practical training and work in engineering. As a pupil of Sir John Hawkshaw in the years 1859-62, he saw and took part in some of the important improvements in the drainage of the Fen districts. In 1862 he became a partner in the firm of Messrs. McClean & Stileman, and in the four following years he lived in the north as resident-engineer of the Barrow Docks and Furness and Midland Railway. For some time afterwards his

attention was occupied with engineering matters, chiefly in London; but in 1870 he retired from his professional business, and went to live at Tunbridge Wells, at Ferncliffe. There his family—three sons and two daughters—grew up around him: he had, in 1865, married the daughter of Mr. John Greg, of Escowbank, Lancaster.

There, too, he embarked upon the various studies and pursuits which remained full of interest for him throughout his life. The spectroscopic side of astronomy from the first absorbed his attention. His 15-inch reflector and his well-known star-spectroscope, devised in 1875, were the instruments with which he first worked. Early in the eighties he was observing solar prominences, but this work was laid aside when he moved to Rusthall House, Tunbridge Wells, in 1884. There his polar heliostat was fitted up in the roof of the house, and with a grating spectroscope and electrical appliances, many of them of his own construction, he began his studies of the solar and metallic spectra.

He built there an observatory with a fine twin refractor, a 10-inch visual coupled with a 12-inch photographic. It was with the latter instrument, provided with a 12-inch objective prism of refracting angle 20° , that he carried out his survey of the spectra of the northern stars. It is interesting to note, as a sign of his vigour, that this survey was begun in the 59th year of his life. After two years' work, unaided by any assistant—for he delighted to work alone—he had finished the northern sky (spectra of 160 stars) and found such interest in it, that in the spring of 1897 he went to the Cape of Good Hope to complete the survey of the whole sky. In six months' time he had secured 292 photographs of the required 116 stars in the southern hemisphere. On the important and interesting results of this large bit of work, the President of the Royal Astronomical Society dwelt when presenting the gold medal of the Society to Mr. McClean in 1899.

Throughout his scientific researches Mr. McClean was prosecuting other studies with keen interest. He clung to his old humane instincts; he studied history from his interest in ancient coins, of which he made a fine collection; French and Italian were mastered that he might have the greater insight into mediæval art and manuscripts; of these, too, he had superb specimens, sought out by himself in Italy, France, and elsewhere. Then, as if this were still not enough to occupy his attention, he found time to work out plans by which he might advance astronomical science by increasing the opportunities of students or the equipment of observatories. But to write of his noble benefactions to astronomy seems almost like doing wrong to his memory; for in these matters it was his delight to see things go forward and not to let anyone dwell on the part which he took in advancing them. He has left us with obligations, which it behoves us to fulfil.

H. F. N.

The November Meteors of 1904.*

THOUGH there was no prospect of a brilliant display this year, there seemed the probability of a pretty conspicuous shower. In 1838—five years after the great meteor-storm of 1833—Mr. Woods, of London, reported in the *Times* that on the night of November 12, between 15^h 25^m and 15^h 55^m, “nothing could exceed the grandeur of the heavens. Meteors fell like a shower of bombshells in a bombardment and in such rapid succession as to defy every attempt to watch their particular directions or to ascertain their numbers.” Mr. Woods estimated that he saw 400 or 500 meteors during the half-hour mentioned.

In 1872 also, about five years after the brilliant displays in 1866, 1867, and 1868, the Leonids returned pretty abundantly, for on November 13, 12^h to 18^h, several observers at Matera, Italy, counted 638 meteors, and the display was regarded as having been much brighter than usual.

In these circumstances it was expected that the return of 1904 would be deserving of careful observation, and so it has proved, though the shower was perhaps not quite so rich as expected. The Earth, however, probably passed through the denser part of the stream at about Greenwich noon on November 15, and thus it must have escaped observation in England. Reports from American stations are awaited with interest. In this country fogs were very prevalent at the important time, and at some places appear to have obliterated the phenomenon.

At Bristol during the night of November 13 there were very few meteors visible, with only occasional Leonids, but the stars were dim in the fog.

On November 14 the conditions were more favourable. Between 13^h 30^m and 15^h 45^m about 55 meteors were seen (including 33 Leonids) by the writer during a watch extending over 1½^h of the period named. It was considered that Leonids were appearing at the horary rate of 25 for one observer. After 16^h increasing fog interfered with observation. The Rev. S. J. Johnson at Bridport had, however, a very clear sky after 16^h, and noted a fairly numerous display of Leonids, including one as brilliant as Venus and several equal to Jupiter. He does not mention the exact number seen.

Mr. C. L. Brook at Meltham, near Huddersfield, watched on November 14 between 16^h and 18^h, and counted 69 Leonids, of which number 17 were observed in the first quarter of an hour. Other results have come to hand which corroborate Mr. Brook's figures, and show that the maximum was attained between 15^h 50^m and 16^h 20^m, when the rate of apparition was one Leonid per minute in the sphere of vision commanded by one observer.

There appear to have been very few Leonids seen either on the nights of November 13 or 15.

* From *Nature* of November 26, 1904.

As observed at Bristol, the radiant seemed to be an area 4 or 5 degrees in diameter, with its centre slightly west of γ and ζ Leonis, or at $151^\circ + 23^\circ$. There were several minor showers visible, and two of these were well pronounced at $43^\circ + 21^\circ$ and $144^\circ + 37^\circ$.

W. F. DENNING.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Standard Time in India.

DEAR SIRS,—

In the number of the *Observatory* for October last (p. 381) there is a note on "Time in India," which states that "it is proposed to adopt as Standard Time for use upon all Indian railways and telegraphs the time of the meridian of $5\frac{1}{2}$ hours east of Greenwich, and for Burma that of the meridian $6\frac{1}{2}$ hours east." This is a distinct departure from the practice hitherto followed, of counting the time according to meridians differing in all cases by an integral number of hours from that of Greenwich, and the proposal to employ meridians involving half-hours of time, in the case of so large and important a region of the Earth as that of India, if adopted, will make an unfortunate break in the existing chain of countries hitherto working on one uniform plan.

On the introduction of railways the use of local time fell everywhere into disuse, that counted in each country being generally the time of its capital city; but, again, as long-distance travelling increased, further inconvenience arose in passing from one country or district to another, by reason of the still remaining fractional changes in the reckoning of time. Owing to the vastness of the American continent the inconvenience was in that country early felt, one, however, that became entirely removed by the admirable arrangement so successfully carried out, some 20 or more years ago, by the railway authorities of Canada and the United States, the pioneers of a system since universally adopted.

A few words as to how the change was effected. Districts were created about meridians differing each by an integral number of hours from that of Greenwich (those of 5, 6, 7, and 8 hours west), the standard time counted throughout each district being that of its corresponding meridian. This was a great step in advance in the unification of time, as by the arrangement mentioned the same minutes are shown on *all* the clocks of the included districts, with change in each district only of the hour.

Conferences had been held at about the same period in Rome and at Washington at which, amongst other things, there was consideration of the question of universal time. That of the civil day at Greenwich was recommended. And the extension of the American system since made in so many parts of the world has practically confirmed the wisdom of that recommendation, the

time shown on *all* clocks being now, as regards minutes, similar throughout nearly the whole world, and in agreement also with Greenwich, with change in each section of the hour only, as before mentioned.

I may perhaps be allowed to add here a little table taken from an article on the time question communicated by me to the journal *Nature* in the year 1884 (vol. xxxi. p. 7). in which I formulated an extension of the American system to some other parts of the world, as follows:—

Countries.	Longitude from Greenwich of meridian to be employed for local standard time.
	h
Great Britain, France, and Spain.....	0
Norway, Sweden, Germany, Austria, and Italy	1 east.
Western Russia, Turkey, and Egypt	2 „
Western India	5 „
Eastern India	6 „
Western Australia.....	8 „
South Australia	9 „
Victoria, New South Wales, and Queensland	10 „
New Zealand	12 „
California	8 west.
Eastern America (Washington)	5 „

Since the publication of the preceding table the principle indicated therein has been adopted in many different countries to an extent that I am unable precisely to particularize, not having at the moment the means at hand for so doing. Neither can I say that the meridians have been apportioned precisely in accord with the precepts of the table, but it will be remarked that I had selected those of 5 hours and 6 hours east of Greenwich for the two standards of time in India. And the proposal to deviate from the principle hitherto followed is the first occasion of departure on any considerable scale from the plan until now everywhere acted upon with such admirable results. For should the meridians of $5\frac{1}{2}$ hours and $6\frac{1}{2}$ east of Greenwich be really employed for Indian standard times, it will follow that India will have on either side, both to the east and to the west, bordering or nearly bordering countries in which the standard time already in use is that differing from Greenwich by some whole number of hours, and, if not immediately, India will ultimately come into contact therewith. When this does occur, the difference of 30 minutes between the standard times of India and those of the adjacent countries, east and west, may in the future prove inconvenient, in the intercommunication between the countries that might arise, in regard to railway, postal, telegraph, and steam-ship services, and certainly would be inconvenient to travellers in those countries.

Yours faithfully,

Blackheath, 1904, Nov. 23.

W. ELLIS.

Australian Observatories.

GENTLEMEN,—

I have been found fault with by a friend for saying in your October number (p. 372) that “for more than ten years Australia remained without an observatory.” The expression was taken from the Introduction to the first volume of the Sydney Observations, where it is remarked that Sir T. Brisbane’s Observatory at Paramatta “continued in operation until 1847, when it was dismantled, after some years of apparent neglect, and for ten years Australia remained without an observatory.” That at Sydney was commenced in 1857, and observations were first made there the year after. But my friend’s objection was that Mr. (now Sir Charles) Todd was nominated to the directorship of the Adelaide Observatory in 1855. It does not appear, however, that he possessed appliances for astronomical observations there until 1861, when he observed the transit of Mercury on November 12 with a Dollond telescope of $2\frac{1}{4}$ inches aperture. That phenomenon was also partially observed at Sydney and Williamstown, but the times of both ingress and egress were recorded at Adelaide, and other observations later, particularly when the equipment was increased.

The colony, however, of Victoria was ahead of South Australia in commencing astronomical work. In July 1853 Mr. Ellery was appointed to organize and superintend an observatory at Williamstown. The site selected was on the low spit of ground which terminates at Gellibrand’s Point. Observations were commenced in 1855, but were confined to determinations of time and of latitude and longitude. In 1861 a transit-circle was provided, with which a series of star-places was determined, but in 1863 the observatory was removed to its present more advantageous position at Melbourne.

Yours faithfully,

Blackheath, 1904, Oct. 17.

W. T. LYNN.

The Nova Ophiuchi of 1604.

GENTLEMEN,—

It is now just three hundred years since the outburst of this remarkable star, which seems to have been first noticed on the 9th or 10th of October. Kepler did not see it until the 17th, on account of cloudy weather about the time he heard of the discovery; but it had been previously perceived by Möstlin, David Fabricius, and others. The three planets, Mars, Jupiter, and Saturn, were near it at the time, and it is said to have exceeded them all in brightness. I cannot quite understand Kepler’s remark that it was brighter and redder (röthlicher) than the dog-star, for though I do not wish again to touch upon the vexed question of the changed colour of Sirius, the Nova was in such a distant part of the heavens that it is not easy to see how they could be compared. No change seems to have taken place in the

star's brightness before it disappeared in the twilight; but when it appeared again in the east about the end of December 1604, it was noted to be less bright than Arcturus, though still exceeding Antares, from which its apparent distance was not great. From that time it continuously diminished in brightness, and though seen early in 1606, it had quite disappeared by March in that year.

The place of the star was in the right or eastern part of Serpentarius or Ophiuchus, nearly two degrees north of the ecliptic. From the observations of Fabricius, Schönfield endeavoured to make a more exact determination, the result of which was that it was situated about halfway between ξ and 58 Ophiuchi, which would be at the present time R.A. $17^h 26^m 28^s$, N.P.D. $111^\circ 19' 4''$. Now Chacornac in 1861 mapped a star of the 10th magnitude about $2'$ preceding this spot, and, attention being called to this by Hind in *Nature* for January 28, 1875, and to the fact that the star in question seemed afterwards to have disappeared, Winnecke looked at the place and noticed a star of the 12th magnitude, very near Chacornac's (*Nature*, August 12, 1875). I have not been able to learn whether any other observation has been obtained since, or whether the region has been photographed. But surely it would be desirable to ascertain whether there is a variable star in the place of the Nova.

Yours faithfully,

Blackheath, 1904, Nov. 2.

W. T. LYNN.

OBSERVATORIES.

[Continued from p. 374.]

HEIDELBERG. *Max Wolf*.—An interesting table of the weather is given. We extract the following:—

Number of fine nights, 1898	134
„ „ 1899	152
„ „ 1900	129
„ „ 1901	173
„ „ 1902	138
„ „ 1903	143

During the year 118 photographs of separate objects were taken with the Bruce telescope, and 132 with the 6-inch. In all 619 plates were exposed; many were for small planets, 41 of which were discovered during the year.

Observations of Comets 1902 *d*, 1903 *a*, 1903 *c*. Photographs of nebulae.

JENA (University Observatory). *Otto Knopf*.—Small planets.

JENA. *W. Winkler*.—Jupiter's satellites; comets. The Sun was examined with the 4-inch on 259 days.

KALOCSA. *J. Fényi, S.J.*—Clouds interrupted the Sun observation, but 233 drawings were made with the projection apparatus. The Sun's limb was examined on 151 days completely, and partially on 23 days. 27 prominences over 100" in altitude, the highest being 249". Meteorology.

KASAN. *D. Dubiago. Engelhardt-Sternwarte.*—Catalogue-places for variable stars. Wide double stars and comets. Meteorology.

The old Observatory. Observations of Comets 1902 *b*, 1903 *a*, and 1903 *c*. Minor planets. Variable stars. Time-service.

NEUCHÂTEL. *Arndt.*—Chronometer and watch testing. The 6-inch Merz has been employed for observation of variable stars. The lunar eclipse 1903 April was observed under favourable circumstances. Meteorology. Time-service.

O'GYALLA. *Dr. N. Thege v. Konkoly.*—A large number of variable stars were observed with a Zollner photometer on the 6-inch refractor. The Sun's surface was observed on 166 days, 25 being free from spots. Meteorology. Time-service. An interesting table of the fine evenings from 1903 May to 1904 May is given; altogether there were 84, the average from beginning of November to end of April being only 4 per month.

POTSDAM (Astrophysical). *H. C. Vogel.*—The 30-inch refractor was used on 60 evenings by Dr. Ludendorff, who obtained 136 spectra of stars with Spectrograph II., and 25 with Spectrograph III. Dr. Hartmann secured spectra of Nova Geminorum, of 3^h, 1^h, and 40^m exposure, the results of which are published in *A. N.* 3858. Profs. Scheiner and Wilsing were working on the radial velocity of the brighter nebulae. The 12-inch refractor was used by Drs. Eberhard and Ludendorff, who secured 206 star-spectrographs. A variety of other spectroscopic work was done, the results of which have appeared in the *Astronomische Nachrichten*. Profs. Müller and Hartwig are engaged in compiling a new Variable-Star Catalogue. Work is being pushed forward on vol. iv. of the 'Astrographic Catalogue.' Prof. Scheiner continued his studies on the temperature of the Sun.

STOCKHOLM. *Bohlin.*—Re-observation of the Radeliffe Stars (1845). The astro-photographic refractor was employed in the observation of variables and nebulae.

STRASSBURG. *E. Becker.*—The 18-inch refractor was used for obtaining positions of nebulae and of Comets 1902 III., 1903 I., 1903 II., 1903 IV.; also for diameters of Mercury, Venus, Mars, Jupiter, and Neptune, for selenographical positions, and for 108 double stars. The meridian circle was used for circumpolars. Zone work, -2° to -6° .

VIENNA (*M. Edler v. Kuffner*). *L. de Ball.*—The zone catalogue -6° to -10° was practically complete in February, and the printing is being pushed forward. Parallax work.

ZURICH. *A. Wolfer*.—Sun observations have been carried on as usual; on 270 days a complete picture of sun-spots. Prominences on 158 days. For sun-spot statistics a 9-c.m. telescope with polariscope was employed on 281 days.

	Days of obs.	Rel. No.	Days of no spots.
1902	276	4·7	198
1903	281	25·3	37

PUBLICATIONS.

PHYSIOGRAPHY*.—There are probably many well-read persons who do not know what the science of Physiography is. It is a subject of the South Kensington Course, and the late Prof. Huxley wrote a book on it, which has been revised and partly re-written by Prof. R. A. Gregory, of Queen's College, London, and a copy of this is now before us. It appears from this that Physiography is the science which teaches how our Earth, as to its contour and plan, has come to be as it is. The original book, we are told in the introduction, was based upon a course of lectures on the Thames and its basin, but the new book transfers the central idea to any river-basin; and from the action of streams, with side chapters on water, dew, snow, ice, pressure, temperature, and so on, the method of formation of the Earth's surface is followed out. The book is profusely illustrated with very interesting views, and it can be gathered from these alone how much Prof. Gregory has added to the original work, for they include pictures of the eruptions of Mont Pelée in 1902 and Soufrière in 1903, of the Indian earthquake in 1897, and of a landslide at Dover in 1897. There is a little astronomy in the book, because Physiography also includes knowledge of the movements of the Earth, the cause of day and night, of summer and winter, and the effects of gravitation, which are all clearly explained, but we read the first part with much more interest.

NOTES.

COMET NOTES.—Several observations of Encke's Comet were obtained at the end of October and beginning of November; it was extremely faint, about the twelfth magnitude, so that it is doubtful whether it will come within reach of small instruments on the present occasion. An ephemeris was given last month.

Brooks' Comet, discovered last April, is still faintly visible, and has been under observation at the U.S. Naval Observatory and

* 'An Introduction to the Study of Nature.' By T. H. Huxley. Revised and partly re-written by R. A. Gregory. London: Macmillan & Co., 1904. Price 4s. 6d.

elsewhere. The corrections required by Nijland's ephemeris on Oct. 17 were R.A. -2° , Dec. $-0^{\circ}4$. The following ephemeris is for Greenwich midnight (*Ast. Journ.* 568, Suppl.):—

R.A.					N. Dec.					R.A.					N. Dec.				
h m s					° ' "					h m s					° ' "				
Dec. 1		12	37	27	48	30				Dec. 13		12	32	1	51	14			
5		12	36	7	49	22				17		12	29	8	52	15			
9		12	34	20	50	17				21		12	25	37	53	18			
										A. C. D. C.									

MINOR PLANET NOTES.—The following new planets have been discovered at Heidelberg:—

Planet.	Date.	Discoverer.	Mag.
PD	1904 Oct. 15	Götz.	12.5
PF	16	Wolf.	13.0
PG	Nov. 13	"	12.8
PH	13	"	12.8
PI	15	"	13.0
PK	15	"	12.0
PL	16	"	12.2
PM	16	"	12.1

PC is probably identical with 485, PH with the long-lost 157 Deianira. A planet named PE is identical with 178 Belisana, OM with 236 Honoria.

394, 499 have been named Arduina, Venusia respectively.

A. C. D. C.

OBITUARY.—It is with unfeigned sorrow that we have to announce that we have lost Mr. McClean. He died at Brussels from pneumonia on the morning of Tuesday, November 8. An appropriate article on his life and character will be found on another page of this number, to which may be added that Mr. McClean has bequeathed £5000 to the Newall Observatory at Cambridge for improving the instrumental equipment, £2000 each to the Royal Society, the Royal Institution, and the Royal Astronomical Society, £5000 to the University of Birmingham, to be applied in the department of physical science; and to the University of Cambridge, for presentation to the Fitzwilliam Museum, he left his collection of illuminated manuscripts and early printed books, and all objects of mediæval or early art which the Director of the Museum may select.

INCLINATION OF THE MOON'S AXIS TO THE ECLIPTIC.—The following curious contradiction has been brought to our notice and may be worth publishing. In the list of dates at the end of Mr. Peck's valuable 'Observer's Atlas of the Heavens,' the inclination of the Moon's equator to the ecliptic is given as $1^{\circ} 32' 9''$. This is the value which was determined by Wichmann in 1848 from observations obtained with the Königsberg heliometer. From

another series at Strassburg, Hartwig in 1880 deduced the value $1^{\circ} 36' 39''$, and in 1881 Pritchard determined it from Oxford photographs as $1^{\circ} 32' 58''$.

Wichmann's value is accepted in Neison's great work on the Moon, which was published in 1876. But, oddly enough, he gives also the inclination of the Moon's axis to the ecliptic, which should be complementary to the other, and gives it a degree in error as $87^{\circ} 27' 51''$. These values, both of the inclination of the equator and axis, seem to be copied into Elger's book on the Moon. Mr. Peck gives $1^{\circ} 32' 9''$ for the former, and instead of $88^{\circ} 27' 51''$ for the latter, transforms it into $87^{\circ} 57' 21''$. The great utility of his work renders it desirable to point this out. It may be added that the value given in the *N. A.* is $1^{\circ} 31' 22''$, and is that found by Dr. Franz from observations at Königsberg.

WE offer our sincere congratulations to the Astronomer Royal, Sir William Christie, who was promoted to the rank of K.C.B. on the occasion of His Majesty's birthday, November 9.

THE next Meeting of the Royal Astronomical Society will be on Friday, December 9; of the British Astronomical Association on Wednesday, December 28.

FROM AN OXFORD NOTE-BOOK.

Mount Wilson, California,
1904, Sept. 2.

KIPLING has a story somewhere of a pious and learned Hindu who renounced a life of success and splendour in the world and sought out a peaceful spot in the mountains where he could sit down permanently and think. After our eventful journey across the continent we feel, in this haven of rest, rather like that Hindu. We are on the very top of a mountain 6000 feet high, which rises straight up from the plain. The sky is clear, and there is no wind—it would be too much to say that it is *never* cloudy or windy here, but such phenomena are rare. Below us lie the cities of the plain—Pasadena and Los Angeles—generally in a cloud of either dust or fog, which softens the details by day; but at night, when the electric lights shine in their streets and on the winding road connecting them, they become glittering lakes connected by a river—a wondrous sight. Round us are other mountains near and far; close by is Mount Lowe with its inclined electric railway, which will take you up the mountain, or stop halfway to shew you the telescope (formerly belonging to Prof. Lewis Swift, now the property of this railroad); but there are many other peaks as yet unoccupied by telescopes.

HERE, in this perfectly charming spot, the foundations of a great observatory are being laid. In making this deliberate asser-

tion I do not for a moment underestimate the difficulties and uncertainties still ahead of Prof. Hale ; but I do not regard them as more serious or more numerous than those which lie behind him. He has already accomplished much ; friendly hands have been outstretched to him in this new country, and he has been presented, in trust for Astronomy, with a large slice of the top of the mountain. Help has been accorded from the Carnegie Institution, and we shall all hope that more will be forthcoming. He has tested the climate and found that he can count on a wonderful persistence of favourable conditions ; and experiments with modest apparatus have given encouraging hopes that a big telescope can be worked. The piers for it are nearly built (it is no small matter to carry materials for some big solid stone piers up 6000 feet), and the housing is on its way. In a month or two it is hoped that the fine telescope presented by Miss Helen Snow will be ready for trial for spectroscopic work of various kinds on the Sun.

THE chief anxiety ahead, assuming that there is no disappointment as regards funds, is the doubt whether a large horizontal telescope can be got to work on the Sun. Are the temperature effects too serious ? For night work on the stars these difficulties do not arise ; and even when using such telescopes at solar eclipses it is possible to screen off the direct sunlight until near totality. But it is another matter to expose mirrors to full sunlight for many minutes together : changes of focus, of a serious kind, are apt to occur ; and it is not yet known whether they can be obviated. One possibility is to have mirrors of fused quartz instead of glass, when it is hoped that the temperature-effects may be rendered insensible ; and experiments are already being made to test this method. Then there is another class of difficulties, connected with the temperature of the air through which the beam of light passes—the radiation from the ground, and so on. Altogether a formidable crop of difficulties, which it may take some years to deal with ; but, either because Prof. Hale has already conquered so many other difficulties, or because it is so delightful to sit here and contemplate the scenery and the future, one is inclined to take a very optimistic view of the outcome. And if such a telescope *won't* work, why, after all, that will be something to know !

WHEN we are so foolish as to go indoors, for sleep or other human weakness, we are luxuriously housed in a future machine shop, with free use of a fine gas-engine if we should require it during the night. The rest of the community lives in a "monastery," with a separate "cell" for each ; and though the present building is only provisional, I gather that the arrangement has been so successful that it has been adopted as the basis of the final scheme. The site for the new "monastery"

is like that of an old German schloss, only more so; it is the extreme point of the summit in one direction, overlooking a cañon which is almost inaccessible from below. There will be an ambulatory; and, of course, a refectory; and a magnificent scriptorium, for which presents of very old MSS. will be welcomed. It has not yet been decided what shall be the name of the liqueur which the monks shall brew (Copernicine?), but some search has been made among the rocks for a suitable wine-cellar. A scheme is under consideration for getting the Sun himself to ring the bell for matins and vespers.

WE paid a visit a few days ago to Mount Lowe. The telescope is at present in charge of Mr. W. H. Pickering, who is nobly relieving the regular demonstrator in return for the use of the telescope (with its climate), when the passengers by the electric railway cease their troubling. He had some interesting things to tell us about the Moon, and about the ellipticity of Jupiter's satellites, but will doubtless prefer to complete his observations before making them public. Some of his lunar observations are rather startling, but how many people have the *knowledge* which would entitle them to an opinion on their merits? Few astronomers are observing the Moon at all, and fewer still have as large a telescope as 18 inches in a fine climate. The Moon may complain of undue neglect recently with nearly as much justice as may the Sun.

WE learnt from Prof. Pickering that one of the small stars plotted at the Yerkes Observatory turns out to be the satellite Phœbe, which is thus amenable to visual observation. How many more of these faint satellites may there not be? It certainly seems as though an outer satellite to Jupiter should be looked for. This seems an appropriate moment to print the following note which I received from a correspondent (W. W. B.) before leaving England:—

“HINTS ON THE DISCOVERY OF NEW PLANETS.

“A recent suggestion for determining the solar parallax from accurate observation of the interval of time between the appearance of a sun-spot and the occurrence of its associated disturbance must now hide its diminished head. Mr. D. A. N. Grover, author of ‘New Principles,’ goes at least one better: we quote a sentence from a small corollary to his scheme of the Universe:—

“Correct observations of the Earth's mean temperature will locate new planets by tracing to them the causes of certain radical changes in the Earth's mean temperature that are not caused by any known planets.

“The basis of the scheme is a universal repulsive force, by which incidentally a planet passing between the Earth and the Sun pushes the Earth away and makes it colder; and conversely exterior planets push it towards the Sun and make it warmer; while smaller variations arise from the oscillation of the Earth within and without its equilibrium distance.

“The Physics(?) are not worth reproduction perhaps, but one likes sometimes to see the connection of ideas, as in the ‘Diminution

of crime owing to the popularity of the motor-car,' where the missing link is the 'misdirected activity of the police.'"

THE work on Mount Wilson at present is chiefly that of construction. Some noble piers are being built to raise the long horizontal telescope well above the ground. The building is costly, for the stone in the immediate neighbourhood was found to be inferior in quality, and the materials are thus being brought from a distance. The "trail" is narrow and steep, and only negotiable by mules in single file. But these amiable creatures sometimes try to crowd past each other; and when the slope is specially precipitous, and there is a human being seated on the mule which is anxious to take the outside route, the journey becomes full of interest. I don't know exactly how wide the trail is; but I know that if it has to be widened to five feet, so that Mr. Ritchey may have his 5-foot mirror carried up, the widening will cost many thousands of dollars. At present Mr. Ritchey is busy establishing a workshop down on the plain in Pasadena, and arranging for the high temperature electrical furnace which shall make the fused quartz experiments. But it is the desire of his life to get that 5-foot mirror up on Mount Wilson and at work, and when he does we may expect something in the way of news.

BUT at present the business of the day is mainly concerned with the gradual raising of the piers, or with the bestowal of what the pack-train of mules, and the wonderful carriage designed to carry the heavier loads up that narrow path, have brought up in their successive journeys. In this rainless clime the materials can usually be dumped down anywhere; but one day we had an alarm of rain (indeed a few drops actually fell) and it was our joy to render aid (however insignificant) in the miraculously speedy erection of a shelter for certain damageable articles, such as the coelostat. A house was literally built "while you wait," or rather while the cook was waiting to serve lunch; but even the cook recognized the exceptional nature of the emergency.

IN the evening we can as yet only gaze at the stars with the naked eye, and meditate on the years to come when the Monks of Mount Wilson will be making their beautiful pictures of Sun and stars and spectra, and filling the whole astronomical world with the desire to make the pilgrimage thither. Or we can read poetry; and we actually do. I shall not reproduce here any of the poems we read aloud, for they were by well-known authors; but I will take the opportunity of mentioning a new poet. From a brief notice in a newspaper I learnt that a scientific poet had arisen, by name Ronald Campbell Macfie, and it was an obvious duty to send for his 'New Poems'*. They are by no means all scientific, but they boldly introduce facts with which some of his readers may not be quite familiar. As a fair sample we may take the opening lines of a poem on "Waves":—

* Published by John Lane, the Bodley Head.

Beyond the blue, beyond the red,
 The waves of ether, fast and slow,
 Against our eyelids vainly beat,
 Vainly atween our eyelids flow ;
 And yet to the clairvoyant dead
 What various colours they may show !
 The colour of translucent thoughts,
 Of golden dreams, and silver sighs,
 The crimson of a human heart,
 That, ever-loving, never dies,
 The blue of the forget-me-nots
 That live in God's immortal eyes.

The waves of sound are next dealt with, and the ideas then used to pay a beautiful tribute to a dead friend. So far as an outsider may judge, there is some really good poetry. We may take another scientific allusion from the address of "the Earth to the Sea," which contains the following lines :—

And through your swelling tides I feel
 The sweet persuasion of the Moon,
 And learn the transcendental law
 Which makes a million systems one—
 The subtle central bonds that draw
 Planet to planet, sun to sun.

But before we have read far, we begin to feel an uneasy suspicion that the poet's sense of humour is not very keen, especially if we tackle a long poem at the end of the book, which points out the imperfections of the Universe generally, and begs the Queen of England to put them right by a word or two :—

And, these things being so, O Queen, our Queen,
 Wilt thou not speak, and save poor foolish men
 From wasting noble lives in vain pursuit
 Of phantom joys ?

But this appeal is not made until a good deal of ground has been covered and various puzzling questions asked :—

. . . . What in very sooth are eyes ?
 Eyes, like the Sun, are nothing save in thought ;
 And not upon the eyes does sight depend :
 A blind man may have light within the brain—
 A blow may make him see a thousand stars.

I can't help feeling that somebody will be provoked to mirth by this last line, which is apparently intended to be quite solemn and serious.

THE only cloud on our horizon at present represents the fact that we must so soon leave this peaceful existence. In a day or two we must go down the mountain again and resume our journeyings. There is the Lick Observatory ahead of us as an allurements ; and beyond that the great Congress of Arts and Sciences at St. Louis, with all the attractions of the World's Fair. But we feel at present as though we would sooner stay here. It is not only peaceful but profitable ; for how much have we not already learnt from these wise Monks ? May success attend their grand mission !

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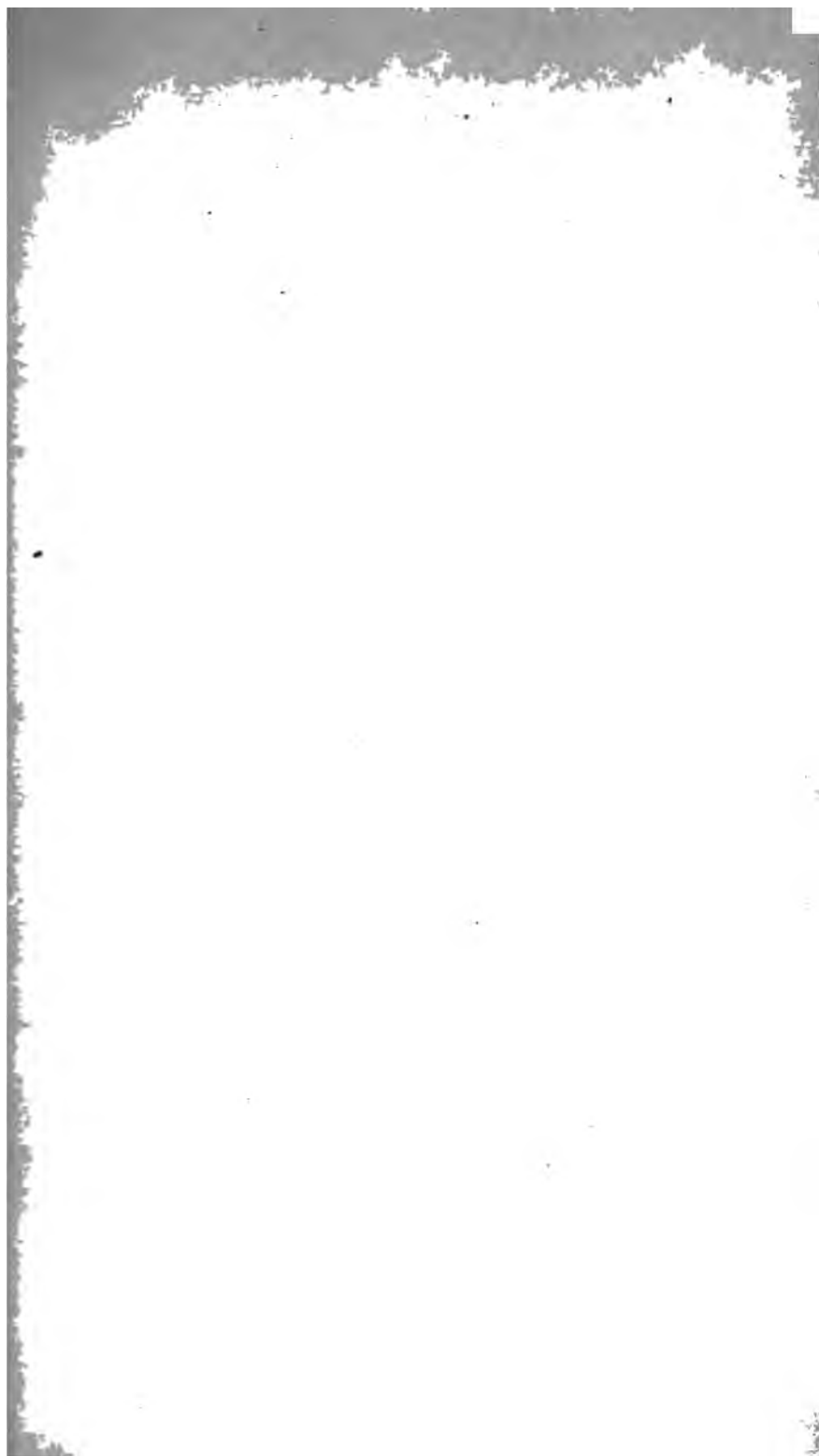
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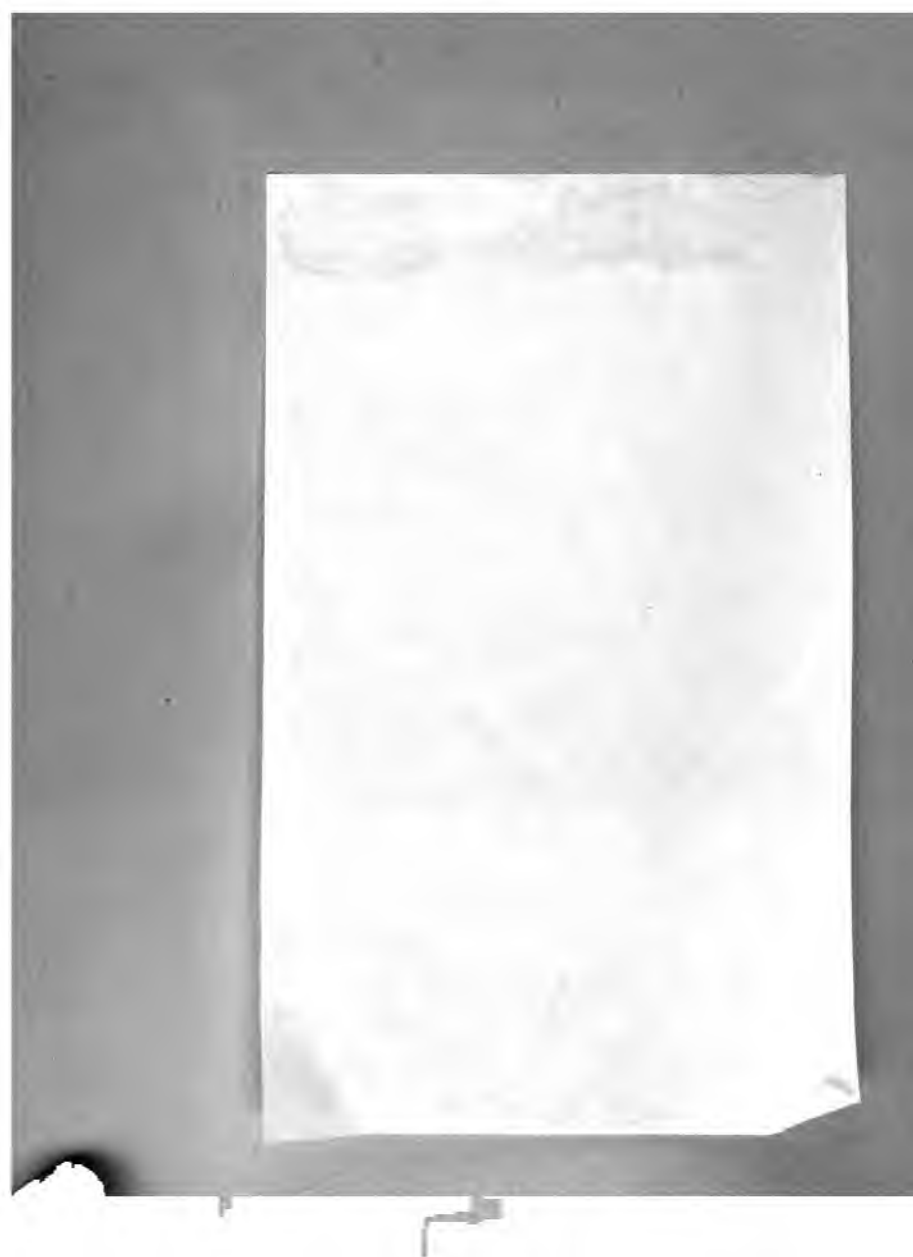
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